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THE SCIENTIFIC VERSUS THE PHILOSOPHIC APPROACH TO THE NOVEL PROBLEM[1]

By Professor TRUMAN L. KELLEY

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IT has been common to differentiate between science and philosophy, crediting the former with reliance upon facts and a kinship with data and the latter with a trust in logic and an affinity for the pure and ethereal verities of the mind. The expression "science proves" carries a very different connotation from "philosophy establishes." If it is held that "science proves" by a sound process, but one that is different from a second sound process whereby "philosophy establishes," I assert that the contention is wrong. The thesis of this paper is that there is but one method tending to establish truth in the world of phenomena. This issue can be reduced to that of the places of data and of judgment in reaching decisions in the ordinary affairs of life. I believe you will all agree that if "science proves" data certainly are involved, and if "philosophy establishes" certainly there is a mind at work, but neither data nor the mind can operate alone. To draw a crude, though, I trust, not inaccurate parallel, we can say that life in the biological sense is the interaction of the atmosphere with the soil. Call the soil "data," the atmosphere "mind," then life is "fruitful thought"—the interaction of the two. Dust does not burgeon upon the surface of the moon or even fly into tornadoes, for there is no wind to stir it, and an atmosphere does not swirl in space where there is no dust or other gravitational field to hold it. We can not think without facts, even though we may have facts without thinking. No person or organization can

[1] Address of the retiring vice-president of Section Q—Education, American Association for the Advancement of Science, December, 1929. A part of this address in modified form was first given before Phi Delta Kappa, New York, 1929.

operate without data, *i.e.*, phenomena of life which are at least to a degree measurable and verifiable.

Of course, I can not prove that the soil on the moon does not burst into bloom, much as I may believe it. The scientific accuracy of that statement is not the point, so grant it for the sake of the illustration. Therefore let us investigate in greater detail the part data or measurable items of knowledge play in careful thinking. Any statistic or objective measurement which fits into any classification scheme whatever, *i.e.*, has meaning, is such a bit of systematized knowledge. In addition to this there are novel items, meaningless at the initial moment, but quickly given meaning by the interpreting mind, in short, their novelty disappears and they present themselves as systematized knowledge. Starting with the novel element in an experience we find it quickly taking on the common feature, or statistic, characteristic. Now let us start with the statistic and see if it migrates, in the mind that apprehends it, toward the novel. The objective measurement once gotten is used, and the specific way in which it is used is the matter of importance to us.

Let me picture a situation in which the measurement has apparently taken the place of judgment in order to raise the question of the place of judgment on the one hand and of the measurement upon the other. The activities of the train dispatcher serve our purpose. He receives over the wire information as to the place and rate of movement of the various trains operating within his division. Two trains moving in opposite directions upon a single track would crash did he not heed the objective measures reported to him and see that one train takes a siding. If the expected communication is slow in coming in he must literally long for it, and when it does come grasp it with joy and confidence. There is no trace of rivalry between his judgment and the facts upon which it operates, though these are so potent in the mind of the train dispatcher that they may be said to dominate the situation. This is so because the executive, by a full and free exercise of judgment, *wills* to heed and trust the objective measure. Such importance as it has, has been given to it by an act of judgment based upon earlier experience adequately scrutinized with very similar measures. Thus we are dealing with a scientific conclusion—one impregnated with human judgment and not devoid of it. Because of this science can never be exact, because it is never free of the element of human appraisal.

Let us carry this thought over into school life. A third-grade child is given an ABC reading test and secures a score of 40 on it. It is also known that the average score on this test for all pupils of the third grade in which this child is located is 30 and that the average for the fourth grade is 40. What will the school executive do in this situation? Promote the child one term, promote him two terms, keep him where he is or demote him? The mere figures that I have given do not answer the question. Surely it is inconceivable that the few facts given, unsupplemented by important other facts and unoperated upon by human judgment, do adequately answer a question of promotion. They can not in themselves be sufficient, and teachers and principals should so affirm. Such objective facts should affect a teacher's or principal's judgment, which is the ultimate arbiter, only in so far as the teacher or principal asserts it reasonable that they should. There are a number of things that should operate in the building up of a conviction of trust or distrust of the ABC reading test scores. First, is the agreement, in one's experience, between the test scores and ability of pupils as otherwise ascertained. Second—as pertinent to the individual case—is any accessory information about the pupil that may be available. Third, is the confidence one places in the sponsors and critics of the test (authors and others). And fourth, and ordinarily by far the least important, is the confidence one places in the test as a result of a perusal of the test items. As a result of these investigations one gives little or great heed to the test score.

The point is that it is the executive that *gives* to the score such importance as it has. If, in his best judgment, based upon all the facts that he has been able to muster, the test is not entitled to an important position in determining promotion, then, in the executive channel that determines promotion, it does not and it should not hold an important position. Perhaps some of you disagree with my statement that under these conditions the test "should not" hold an important place. You may say, "Suppose the test is intrinsically an excellent one, then it should hold a high position even though the executive is unaware of its genuine merit." I disagree with this view, for the executive should be the responsible party in the matter and personally held to account for any mistakes. We may hope that it will always be impossible to shift the onus of poor classification to so inanimate a thing as a score on a test.

Unless the executive looks upon the test score as a friendly and serviceable item of information for his own understanding of the child he should not use it. If you are a teacher and learn that little Bessie Jones has weak eyes you will place her in a favorable seat. You are glad that you have this information. As a result of it you and Bessie are better friends—you a better teacher and she a better pupil. The measure of Bessie's eyesight is a friendly fact because you know its implications and it does not mislead you.

Test scores which do not mislead hold the same possibility, making for mutual understanding and friendship between teacher and pupil.

It has been my pleasant duty to test many children. Time and again I have had before me scores for some child upon tests in which I knew from long experience a certain confidence could be placed, and I have literally longed to meet the child in person. Trusting the test scores, not implicitly but to a degree, I felt that I already knew the child, that he was a friend of mine, and I wanted to know him still better—to know wherein the tests had not been quite fair to him and still more to know those reaches of his character about which the test scores had been annoyingly silent.

Treated in this manner the test score is never a substitute for judgment. It is merely an aid in making judgments. It never delimits character, capacity or achievement. It merely helps in the understanding of certain limited portions of these things. It should never circumscribe one's field of effort to understand. While illuminating a limited field it should challenge one to explore the reaches of mental life that stretch beyond.

Suppose a man possesses a small rowboat enabling him to explore the ocean throughout a radius of twenty miles. This does not decrease his knowledge of the ocean entire. On the contrary, it challenges him to get a ship and move out further and to listen to what travelers have to say and to appraise their tales with a sounder judgment. Just so should the information given by one good achievement or mental test enrich one's consciousness of and interest in the subject's life entire.

Certain opponents of objective mental measures assert that tests have a deadening influence upon the curriculum by tending to limit teaching to the narrow fields represented by the tests themselves. This may be so if one becomes so enamored of the test that he loses sight of the child tested. I fear there are such people. I would criticize them, not the instrument they use. In an earlier generation such people were fetish worshipers. Some talisman, some rabbit's foot, some hocus-pocus answered every need. If a storm arose at sea, abracadabra stilled the waters; if a child was sick, abracadabra allayed the fever; if a male heir was desired, abracadabra turned the trick. So to-day, in a somewhat refined manner, we find the believers in the alpha-omega omnibus test. It tells what is good for backward babies; it clears the fevered brow of the dean when the rough-house rowdies walk the carpet; it sheds a great white light when college recommendations are called for, and it does a score of other things as well. You can pick out the modern measurement fetisher by the multitude of widely different things which the test of his choice will do for him.

If a test is in truth a good test it is good for something, not everything. Do we find in any other field of scientific endeavor an instrument that is good for everything? Thermometers measure temperature, barometers pressure, ammeters electrical current, and so it goes. As a thermometer an ammeter is a total failure.

I believe that we should approach any mental measure devised with the idea that its field of utility is limited, but I would be the last to attempt to limit the field by *a priori* considerations. We must by careful trial determine the limits of utility. If we find that they are broad let us keep the instrument with its broad implications. This is not fetish worship—it is knowledge. My criticism is of those who extend the field of application without knowledge. It should be obvious to every one that, as with every other scientific instrument, the judicious use of a test is something achieved only after careful study and much experience. Be confident that the process can not be shortened. Recently a student registered in my beginning class in measurement called me to task for dealing with averages, medians, age and grade norms when all she wanted was to know how to use tests in vocational counseling.

Though my remarks suggest a limit to the field of utility of objective measures I hope they also show the reality of the value of such measures. The score of an individual does not operate of its own accord, but only *via* the mind of an executive who concludes that the case in question properly falls in the class wherein such scores are useful. I have taken much time to make this point, but I believe it is fundamental and, unfortunately, sometimes overlooked. No matter how well fortified by a long past history a certain type of measurement may be, it must take on the characteristics of a novel event in order to be properly interpreted in a new situation. The breadth of view and caution demanded of the test devotee is of the same order as that demanded of any scientist working with specialized measurements of any sort.

To generalize: It matters not whether we think of the interpreting individual as viewing all the elements in the case as novel or as viewing all the elements as lying within his organized knowledge. Whatever the view, the so-called novel or the so-called old elements all call for the same critical appraisal. If the elements felt to be novel are not subsumed under some existing-in-the-mind system, terror, or the taking-a-chance type of decision, results, and if the elements felt to be old are subsumed under an old system without a new and specific vindicating judgment there results a decision characterized by formality and lack

of adaptability. The adequate mental process, whether that of philosopher or of scientist, scrutinizes the felt old and the felt new as though each were both old and new.

Ordinarily a decision, though made in the light of an executive's entire experience, must be promptly made and can not wait to be verified by a time-consuming study. Herein we may look for a difference between the philosophic and the scientific mental process. I will quote the distinction that Dr. Kilpatrick draws in this connection.[2] He states that science ordinarily postulates the question, "If I do this what will happen," but that philosophy can not wait to see what will happen. He says, "Philosophy in contrast faces a situation of necessary action. . . . Note that any situation confronting is actual and must be met, and that any choice or course whatever, including refusal to act, is *an* answer which carries with it its appropriate harvest of consequences. Philosophy then asks, 'In the light of all this what shall I do?'" I believe that Dr. Kilpatrick has here stated *the* essential difference between science and philosophy —other differences follow therefrom. Philosophy is willing to attack any problem, any time, anywhere, and give an immediate answer. Science is not. I would not cite this as to the credit of either the one or the other, but do cite it with Kilpatrick and Dewey[3] as a difference of great moment.

The philosophic question, "In the light of all this what shall I do?" may be paraphrased without inaccuracy as, "In the ignorance as to consequences that enshrouds me, what shall I do?" for, of course, the "appropriate harvest" is not known at the time the decision and resulting act is made. The philosophic answer to an issue is, "Do something and the best you can," while the scientific answer is, "If in doubt delay decision and investigate." Each procedure has its place in this life, this hurly-burly in infinite time. It is something of a travesty upon the mind of man that it is philosophy, sometimes thought of as the enduring, that responds to the hurly-burly, and it is science, changing science, that seeks to be judged by the standards of the permanent. Let us note some of the consequences of these outlooks.

The scientist procrastinates decisions, is other worldly, is of little aid in time of stress. He functions where deliberation and experimentation are possible, and his method is that of experimental analysis, synthesis and verification. On the other hand, the philosopher provides an almost immediate solution. He counsels in times of stress and rides every emergency. His method is that of inadequate analysis, because logical only and not experimental, and inadequate, but much synthesis. Knowing that he has a unique situation to deal with he makes much of "integrations" and "total situations." His total picture, to which he reacts, may be grossly at variance with the real[4] total situation present, but of this he knows nothing because no experimental synthesis of factors has been made.

The statement that[5] "the [philosophical] effort is, as far as may be possible, to find a course of action which will save all the interests, which will integrate all into one course of action that best saves all" is also an excellent statement of the purpose of multiple correlation and of any scientific attempt to explain total outcomes. Though science moves more slowly and with greater assurance here, not being free to synthesize except as experimentation gives warrant, still it moves with the same purpose as does philosophy. In connection with this issue Dr. Kilpatrick implies that science deals only with parts of situations, while philosophy deals with them entire. Now there is no logic that deals with wholes as wholes. If a problem case involving a cross-eyed, untrustworthy, brilliant, crippled, butcher's boy presents itself, what technique can treat this as a whole and without analysis? A logical or experimental analysis must be incorporated in any reasonable attempt to arrive at a solution of the total problem. How will you ever get the case referred to the oculist and how, unless there is analysis, will you ever get the cooperation of the father unless you call upon the butcher, etc.? Philosophy at its best must involve very detailed logical analysis followed by equally careful synthesis. As practiced, and, one would think, even advocated by Dr. Bode[6] in his recent work "Conflicting Psychologies of Learning," the chief emphasis should be upon synthesis. Now experimental science, or that which involves the checking of an hypothesis against an outcome, depends upon analysis as a major feature in the process of arriving at the truth. Why is there a short-circuiting of this step in the philosophic approach? Perhaps a parable is in order.

There was once a very wise man who put a cat in a box with mice outside, and every time the cat scratched its ear, lo and behold, the box opened, the cat jumped out, and the one mouse caught tasted good. As time went on the ear-scratch movement decreased, almost to the vanishing point, but the jump through

[2] W. H. Kilpatrick, "The Relations of Philosophy and Science in the Study of Education," *School and Society*, 30: 39–48, July 13, 1929.

[3] See Kilpatrick, *loc. cit.*

[4] Meaning, of course, not some "thing in itself," but a thing as conceived to exist after very careful study. "Reality" in an ultimate sense can have no scientific, *i.e.*, verifiable, meaning.

[5] Kilpatrick, *loc. cit.*

[6] Boyd H. Bode, "Conflicting Psychologies of Learning," p. 231, 1929.

the door continued to be followed by some plaintive squeak, "The brave cat caught me." The sad part of the tale is that the cat never learned that the wise man had so fixed things that in general the more efficient the ear scratch the bigger the mouse caught, for any squeak nearly convulsed him with joy.

Is philosophy satisfied with any outcome? Dr. Kilpatrick[7] thinks not, but upon this point I fail to follow him. He writes: "having answered, philosophy awaits the outcome to test the validity of its answer as truly as does science." How can philosophy await the outcome as truly as does science? The philosophical act must be terminated at some time, just as must the scientific, and, as previously pointed out, philosophy can not wait—no, its act is terminated when conduct commences. Let me give a case which might easily have grown out of the philosophy of but a few generations ago. Suppose that philosophy concludes that bleeding is good for anemia, a vein is cut and the patient dies, then surely philosophy is to be charged with the death. No spiritual apology will convince a regretful wraith that the philosophic act is still in process. True, had the case been given to science she might have shirked the task, pleaded ignorance and kept her hands off and said, "Let philosophy have the credit." While admitting this penchant of science to procrastinate, still, when a judgment, whether scientific or philosophical, is passed resulting in decisive conduct the problem as originally set is terminated. I must conclude that philosophy does not await outcomes—it acts—whereas science does await the outcomes of its experimental set-ups before it acts in the non-experimental, or important, life situation. If we run through the steps in the complete act of thought, much as given by John Dewey,[8] except that I have added a final step, number 8, we can clearly locate a difference between philosophic and scientific thinking. The steps are:

1. A felt difficulty.
2. A definition of the difficulty.
3. A tentative solution.
4. A mental elaboration of the solution, leading to
   a. Additional tentative solutions and elaborations, if felt necessary, finally leading to
5. The belief that the solution is all right.
6. An experimental verification.
7. An appraisal of the experimental findings leading to acceptance of mental solution and a decision for immediate conduct, or to rejection and a reinstatement of a felt difficulty. The process is continued until a verified solution which is immediately serviceable is obtained.
8. A forward look, or mental picturing of future situations to which the present solution is pertinent.

[7] Kilpatrick, *loc. cit.*
[8] "How We Think," 1909.

The first five steps are common to philosophy and science, but the sixth step, experimental verification, requires appreciable time not available to one who must act. Science continues and carries through the complete act of thought; philosophy does not. The distinction here made is reflected in the difference in activity of acknowledged scientists and philosophers. A distinction which I have occasionally heard calling the thinking part of the scientist's endeavors philosophic and the measurement and manipulative aspects scientific is a distinction that would not occur to or appeal to a scientist, as it would chop him up into unrecognizable parts. There are three important consequences of the difference noted. The philosophic solution is timely no matter how urgent the problem; the philosophic solution is more likely to be wrong than the scientific, and third, the philosophic forward look should be one of misgiving and largely a query.

My mind reverts to the sad parable of the cat. To the philosopher any outcome of his cerebration suffices. There is no conceivable method of determining the real excellence of a proposed course of action in a novel situation at the time it is first proposed. The pragmatic test is how it actually works out, but by the time this test is made the solution given by philosophy is long past. We seem to have reached the conclusion that the only solution to the urgent novel problem is the philosophic one, and that it is no solution because its fitness is and must remain unknown until it is too late to alter it, *i.e.*, until consequences, good or evil, have actually followed. In the strictly logical sense that every present moment is a novel one and that something immediately takes place in reaching the next moment, I believe that this is true, but do not draw the important conclusion that therefore, in immediate problems, we should not turn to science, but resign ourselves to the unverified speculations of philosophy.

That these are unverified Dr. Bode appreciates, for he says,[9] "The more we emphasize man's power to shape his own destiny, the more necessary it becomes to recognize the possibility that he will make a mess of it." Though he realizes this his only comfort is philosophy, for he writes:

> In any event the problem [of whither we are headed] calls for an interpretation and organization of values, which is not a problem for scientific research, but a problem of philosophy. It is not a problem for science because it is not a problem that lends itself to the application of scientific technique. The scientist has his own special devices for collecting and interpreting data, but these devices prove inadequate when the situation calls for a recreating or reinterpreting of old values and old ideals. . . . If the foregoing discussion is correct, then

[9] Bode, *op. cit.*, p. 300.

most of the industry called the "scientific determination of objectives" is on a par with catching birds by putting salt on their tails.

With this I utterly disagree, for the problem of our educational and social ideals is not a problem that must be solved upon the moment. If so solved it would be unsatisfactory even to the philosopher, for to-morrow brings another moment. Far better that a year, a decade, in some matters a generation, be spent in determining educational and social objectives and techniques found by trial to be in line with past progress than that we "settle" the problem by speculation. If one asserts that what constitutes "past progress" is beyond comprehension he is indeed a pitiable optimist if he nevertheless believes that he can define "future progress." We can not settle the problem of objectives for long by any method, but the ten-year experimental study holds promise of fitness and permanence not to be expected in the cloistered solution. Philosophically every moment is novel and calls for a new interpretation. Practically, every moment is surcharged with physical and emotional settings that have sprung from the past and that have been characteristic of innumerable past situations. The novelty of the moment, though real, may be insignificant in importance in comparison with the non-novel elements that are present. One of the features of the moment is the novelty in the organization of non-novel elements. From the view-point of the gestalter this dominates the situation. This is just a point of view and a very unhappy one, for it can never be proved, for, according to hypothesis, no two situations are alike, and accordingly a testing out and verification is never possible. The scientific point of view is to look upon the novel situation as characterized by certain cue or critical elements, or critical combinations of elements, which can be reinstated. The beauty of this view is that it can be proved right or wrong, as the case may be, by trial. This view-point has engendered mighty advance in the physical and biological sciences, and surely psychological and social advance lies in the same direction, for only thus is verification of progress possible.

I have made no distinction between science and philosophy on the basis of remote purpose or outlook and of course subscribe to the idea that a mind-body dualism is not necessary to a scientific point of view, and also to the idea that there is great need of and value in a criticism of historic conceptions. Dr. Kilpatrick considers that[10] "the need for the continual criticism of current thought assumptions in the light of their wider bearings would of itself, apart from all other considerations, suffice to give to philosophizing a permanent place among the higher services of thought to man." The stimulus to evolution consequent to this service can hardly be overestimated, but is it not above all a service rendered by science? From Roger Bacon to Einstein the great cues to a criticism of current thought assumptions have come from science. Philosophy has tagged along and consolidated these scientific salients, but she has not made them. Did the score of philosophical interpretations of relativity precede or follow the experimental findings? Of course they followed. Did the philosophy of Bertrand Russell instigate companionate marriage or did popular interest in it raise an issue that he attempted to place in a rational understanding of mankind? Undoubtedly it was this latter, and so it goes. The intense and keen, but orbital arguments of the middle ages illustrate the value of a criticism of current thought assumptions not leavened by new scientific facts. That science should be the leader in this is inherent in the process of science, for when the verification step of experimental science fails to yield a check with hypothesis there is, practically speaking, an inevitable reexamination of the premises, which of course are merely current thought assumptions. There is no comparable cue stimulating the questioning of assumptions in philosophy. The jolt that sets off the questioning process is from the outside—it is unanticipated experience.

My advocacy of experimental investigation in the attack upon problems of social value is because of the method of science, not because it is exact. There is no "exact science." A definition of science not permitting of error both in the data and in the judgments of human beings who interpret scientific facts is of no practical use. While upon this matter of error we may say that it is scientific to know that there are different degrees of exactness inherent in different stages of an argument. Science recognizes this time and again when philosophy (as practiced) ignores it. Having discarded any thought of exactness in science we can then turn to the important idea of the reliability of measures, findings and judgments. Philosophy has been slow, to its detriment, in following science in this. Until philosophy attaches probable errors to its concepts it will fall far short of its possibilities. In recent generations it has willingly accepted the findings of its younger brother, science, but it should go further and adopt concepts of methodology from it also.

I wonder if there is a conspiracy among philosophers to belittle science and claim its peculiar merit for themselves. Whitehead writes,[11] "There will be

[10] Kilpatrick, *loc. cit.*

[11] Quoted from Dr. Kilpatrick's article. A. N. Whitehead, "Science and the Modern World," pp. 69 and 82, 1925.

some fundamental assumptions which adherents of all the variant systems within the epoch unconsciously presuppose. Such assumptions appear so obvious that people do not know what they are assuming because no other way of putting things has ever occurred to them." Again, "A civilization which can not break through its current abstractions is doomed to sterility after a very limited period of progress." I take these words, not as Whitehead meant them, but as a fine argument for science. To discover errors it only requires that experimental setups involving hypotheses based upon these unknown errors be attempted. Then as the attempt fails the error is revealed.

To what fields must science be limited? Specifically, should some one say that education is outside the pale, would he also say the same of law, sociology, economics, psychology, biology, geology, chemistry and physics? The relationships of physics and those of man to man are of the same order in consciousness. I can not see how one can deny dualism and still draw a line in the list just given separating the scientific from the non-scientific. If one draws no line and concludes that physics can never become a science (a little surprise for the physicists) I for one shall agree, in the sense that as far in the future as the mind can dimly perceive physical issues will arise for which no scientific answer is available—*i.e.*, no answer experimentally and adequately tested out. In the same sense only do I grant that education can never become a science.

Let us sum up the place of philosophy in life and specifically in education. It seems to hold an actual position which I deem to be different from its legitimate one, so let me speak of its actual position first. I will illustrate by reference to a field with which I am familiar—that of test construction. An author devises a test of honesty, let us say. He philosophizes most armchairishly, not failing to consider the experimental evidence reported upon earlier tests having something to do with honesty. He counts his hour or two (or ten or twenty, if you like) of armchair agitation as high grade and adequate for the solution of the issues. So he publishes his original contribution for the use of an expectant world that will no longer wait. True, it may be only that fraction of the world represented by the author that will no longer wait. However, this inability to wait is definitely a part of the philosophical attitude that never matures into the scientific attitude. The decision by the author that his exercises test honesty has been reached by speculation in lieu of investigation. Here philosophy is merely a makeshift, quite unnecessary, for science. This is my lesser criticism of it.

If there is necessity for prompt action any sort of a shift is welcome, so it is not disparaging to say that philosophy is the best method for the expeditious selection of makeshifts. Surely, excepting habitual acts, the majority of the acts of life will fall in this class.

My major criticism is that the test author, having made his speculations and come to his final conclusions, feels very contented with himself—he thinks he has done something worth while and of lasting value. Unfortunately, as he writes fully and with utter sincerity, many of his readers think so too, and thus they also are content and may so remain for years if they attempt no experimental verification of the test. Philosophy is here the great narcotic, the soothing-syrup for author and readers, when in truth a gadfly is needed. It has played this rôle throughout history and it does so to-day.

What is the merit of the philosophical conclusion? Because it is a makeshift it has a place—just the important and unavoidable place of the expedient. Think of a forty-niner in his rush to the gold fields of California in his eastern Conestoga wagon, and suppose that he break a whiffletree out in the Nevada desert. If he is the kind of a man who will not use a makeshift and sends back for a new whiffletree, he might by some be called a scientist—I would use a less complimentary term. Whatever he is called would not apply for long, for he would soon be a scrap of dried bones. In this case the solution will not wait. The time necessary to carry out the complete act of thought, with its step of experimental verification, is not present. Something must be done promptly, and when done it may be called a philosophic solution. This does not state that it is a "right" solution, or even a serviceable one. If the forty-niner tries to mend his whiffletree with a piece of yucca he will probably waste time and increase his danger. If he break up his wagon seat and use the timber he may pull through. Whatever he does has one indubitable merit—that of promptness. There is no certain merit in it on the basis of long-time adequacy. This is characteristic of every philosophic solution. The scientific solution is, or rather aims to be, a more or less permanent solution. The philosophic solution frequently should not even aim to be this, for when such is aimed at there is commonly time for the try-out step, so that experimental verification can enter in and the solution become scientific.

Problems demanding immediate answers, or at least answers before adequate investigations can be made, will always be with us. We shall always need philosophy. There can be no issue here. Not only so—we are going to need it increasingly in the future. If the area of a small circle represents scientific knowledge its periphery may well represent unsolved issues

which, when first met, will ordinarily demand a philosophic solution. With the increase of this circle as science advances goes an increase in the periphery. I neither hope nor look for a decrease in philosophy as science advances, but just the reverse.

We shall need more and above all better philosophy. It seems to me that the most adequate philosopher will have the following characteristics. He will be a man of wide culture, familiar with the arts and sciences, with the psychology of man and with the values of life. He will be an accurate thinker—a sound logician—and have an extensive acquaintance with the facts and the methods of science. There is a peculiar necessity that he be aware of the scientific method. This method aims to secure more permanent solutions to its problems than does any other. It accepts the fact that time and investigation are necessary to this end. Though philosophy acts where these things are impossible the philosopher should attempt to parallel in his thinking what the experimentalist does in fact—only so can there be a tolerably promising philosophic solution of the problem.

Let me illustrate this by a problem which arose during the war. It was necessary to select men for training as officers. It seemed evident that the best selection would depend upon possession by the men of certain traits such as mental ability, physical stamina, moral courage, cooperativeness, leadership, etc. Ratings upon these traits by superior officers of men in camps could be gotten. The problem was how to combine them into single gross ratings which could be used in the actual selection of men. Time prevented an experimental investigation, so philosophizing had to be appealed to. An experimental investigation in which these various trait measures were used to estimate demonstrated success as officers would have yielded the weights that should be attached to the measures separately in order to get the most reliable aggregate measures of fitness as officer material. In short, the experimental treatment would have analyzed the data and then combined the separate trait scores into the most meaningful total ability scores. The concepts of total correlation and of partial correlation (not of course assuming any limited type of relationship, as that of linearity, between measures) here operate and they alone do operate. No logical treatment not paralleling this can be as adequate as one which does parallel it. The more completely the philosopher parallels in his thinking the analysis and synthesis which the experimental treatment would yield the better is his philosophical solution. The great endeavor of the philosopher here should be to ape mentally the steps of science. He can not have a technique which is better (omitting the time factor) than the scientific technique. Just the moment that he demonstrably did have science would claim it as its own, for true science has no fetishes that it clings to in the face of evidence.

CONTRIBUTIONS TO PHYTOPHARMACOLOGY OR THE APPLICATIONS OF PLANT PHYSIOLOGY TO MEDICAL PROBLEMS[1]

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INTRODUCTORY

As early as 1921, while studying general laws underlying physiological and pharmacological reactions, the author conceived the idea of making a comparative study of the action of various drugs on living plant protoplasm in contrast to animal protoplasm. The study of pharmacology, as carried on in our medical schools and research institutions, concerns itself almost exclusively with the effects of drugs and poisons on animals and, strictly speaking, is zoopharmacology, just as the study of physiology in the same institutions is confined exclusively to the physiological functions of animals, or zoophysiology. There is, however, a very important department of biology dealing with plant physiology, to which the name of phytophysiology is sometimes applied, and the writer was curious to inquire into the effects of drugs and poisons on living plants and in this way developed a new department of biology, to which the term *phytopharmacology* might be applied. These studies were begun with an examination of a number of alkaloids, the most interesting of which happened to be cocaine, and the findings in connection with a study of this substance were of such importance that they were followed by other studies along novel and unexpected lines of experimentation, resulting in important discoveries not only of purely scientific value but also of practical worth to pathology, medical diagnosis and therapeutics.

[1] Read before Section N—Medical Sciences, of the American Association for the Advancement of Science, at the Des Moines meeting, January 1, 1930.

The effects of drugs and poisons on plant tissues may be studied in various ways. The author and his associates have investigated the effects of various substances on the germination of seeds, on the growth of roots and stems, on the influence exerted on flowers, the effects on geotropism, on transpiration and respiration, the effect on yeast and fungi, on chloroblasts, on protoplasmic streaming, etc. It was soon found that a most useful and profitable method of making such phytopharmacological studies was the employment of seedlings of *Lupinus albus* and observations on the growth of their roots and stems in normal plant-physiological solutions, on the one hand, and on solutions of various drugs and poisons in the same kind of plant-physiological solutions, on the other hand. In this way the author has developed a quantitative method of studying the phytopharmacological and phytotoxic effects of various chemicals which proved to be of great usefulness.

Studies on Cocaine

It is well known that the cocaine molecule can be easily split by hydrolysis into three components, a complicated nitrogenous base known as ecgonin, a molecule of benzoic acid and a molecule of methyl alcohol. In previous studies, carried on by Macht and various collaborators, the pharmacological effects of cocaine, as such, and other products of decomposition on various animal and animal tissues have been investigated, and it was found that the alkaloid cocaine is very much more toxic for animal protoplasm than the products of its decomposition are.[2,3] A study, by Macht and Marguerite Livingston, of cocaine and of ecgonin, sodium benzoate and methyl alcohol, individually and in combinations, on living plant protoplasm revealed that whereas the alkaloid cocaine is extremely toxic for animal protoplasm, it is comparatively little toxic for plant protoplasm, as much as 2 per cent. cocaine or more being required to inhibit the growth of *Lupinus albus* roots completely. On the other hand, while sodium benzoate, one of the products of cocaine hydrolysis, is practically non-toxic for animal tissues, it was found to be extremely toxic for living plant protoplasm, as little as 0.007 per cent. completely killing the seedlings of the same plant.[4]

These findings, interesting in themselves, revealed that plants may be much more sensitive to certain drugs than animal protoplasm may be to the same drugs, and suggested the idea of employing plant-physiological methods for the detection and study of certain toxic substances not demonstrable by ordinary zoopharmacological or zoophysiological methods. The first extensive study on this subject was carried on by Macht and Lubin in connection with the poison of menstruation.

Studies on Menotoxin

The authors undertook to investigate scientifically by experimental methods, accurately controlled, the old belief in regard to contagion of menstruating women. Studies were made with the serum of women during menstrual and intermenstrual periods, and it was clearly demonstrated by phytopharmacological methods that such blood serum contained a toxic substance—"menotoxin"—demonstrable immediately before the onset of catamenia and during its course, which toxin was extremely poisonous for living plant protoplasm. The presence of the same menotoxin was demonstrated by the authors to be not only in the blood serum but also in the saliva, sweat, milk, urine, tears and other secretions during the menstrual period. By means of phytopharmacological experiments, some idea has also been gained in regard to the chemical nature of menotoxin, it being found that it is probably in the nature of an oxycholesterin.[5] Later studies by one of the authors, carried on along the lines of experimental psychology with albino rats in the circular maze, showed that menotoxin affected the behavior of such animals, thus throwing some light on various psychological disturbances frequently encountered in patients at the time of menstruation.[6]

These studies on the poison or toxin of menstruation, while interesting in themselves, from the ethnological, physiological, pharmacological and chemical points of view, led to a still more important discovery, namely, the first demonstration of a toxin in the blood in pernicious anemia.

Studies on Pernicious Anemia

While making a routine examination of bloods obtained from clinical laboratories in Johns Hopkins Hospital and elsewhere in connection with his studies on menotoxin, Macht had an opportunity of examining a specimen of blood from a man suffering with pernicious anemia. The serum of this patient was found to be extremely toxic for plants as compared with normal blood serum. Further studies revealed that such a toxic reaction was exhibited by blood specimens from every case of pernicious anemia examined by the author, and furthermore, that such a toxicity was not exhibited by specimens of blood from other blood diseases such as secondary anemias

[2] Macht and Bloom, *Arch. Internat. Pharmacodynamie et Thérapie*, 25: 379–390, 1920.

[3] Kubota and Macht, *Jour. Pharmacol. and Exper. Therap.*, 13, No. 1: 31–44, 1919.

[4] Macht and Livingston, *Jour. Gen. Physiol.*, 4, No. 5: 573–584, 1922.

[5] Macht and Lubin, *Jour. Pharmacol. and Exper. Therap.*, 22, No. 6: 413–466, 1924.

[6] Macht and Hyndman, *Proc. Soc. Exper. Biol. and Med.*, 22: 208–209, 1925.

from various causes, carcinoma, Hodgkin's disease, hemolytic jaundice, myelogenous and lymphatic leukemias, etc. As a result of such phytopharmacological studies, a contribution was therefore made simultaneously to the etiology of pernicious anemia as well as to the differential diagnosis of that dreadful disease.[7] For a number of years, the author has been carrying on such diagnostic studies in doubtful cases submitted to him by clinicians from various parts of the United States.

These studies on pernicious anemia furnished a quantitative method of evaluating the results of various therapeutic procedures employed in its treatment and have led to the discovery of certain very valuable therapeutic procedures.[8] Thus it was found that transfusion of blood in pernicious anemia patients is of very little value, the toxin being regenerated in a few days. Again, it was found by Macht and his coworkers that the liver treatment of pernicious anemia, while being of great usefulness in restoring the normal blood picture of the patients, does not affect much the quantity of pernicious anemia toxin present in their blood, so that such patients invariably have relapses. To use an expression of a German author, the liver treatment improves the "anemia" but does not destroy, or affect to any great extent, the toxic agent producing the disease.[9] On the other hand, studies carried on by the author, at first in the laboratory with blood sera in quartz tubes and later on patients, have revealed that certain ultra-violet rays greatly diminish and even completely destroy the toxin of pernicious anemia and, as a result of these experiments performed by Macht and Anderson, it was suggested that heliotherapy with ultra-violet rays of wavelengths 3130 and 2967 Ångström units be employed in the treatment of pernicious anemia, either alone or supplemented by the administration of certain photosensitizing agents and, particularly, of the sodium salt of tetra-bromfluorescein, or eosin. A series of clinical cases carefully studied in this connection has demonstrated the undoubted value of such treatment.[10] Furthermore, the author's findings concerning the toxin of pernicious anemia and the effects of liver treatment have been confirmed by various investigators in Europe.[11,12]

[7] Macht, *Jour. Pharmacol. and Exper. Therap.*, 19, No. 1: 461–469, 1926.

[8] Macht, *Jour. Amer. Med. Assoc.*, 89: 753–759, 1927.

[9] Jungmann, *Klinische Wochenschrift*, 7: 441–445, 1928.

[10] Macht and Anderson, *Jour. Pharmacol. and Exper. Therap.*, 34, No. 4: 365–389, 1928.

[11] Tscherkes, *Proc. Soc. Exper. Biol. and Med.*, 26: 869–871, 1929.

[12] Adler, Sinek and Reimann, *Zeitschrift f. Klin. Med.*, 110, No. 3: 309–333, 1929.

The author wishes to announce, on the present occasion, a new set of experiments conducted by him in connection with pernicious anemia, namely, on the phytopharmacological effects of spinal fluid. A large number of specimens of human spinal fluid have been examined in order to determine the normal toxicity of such fluids. Study is now being made of spinal fluids from cases of pernicious anemia. Such specimens are rather difficult to obtain, but the observations so far in hand reveal that spinal fluid of pernicious anemia patients is definitely more toxic than normal spinal fluid, and this finding may throw light on the origin of the spinal lesions and symptoms so often encountered in such patients.

Studies on Leprosy

The author wishes to announce some interesting studies which he has been conducting on the blood sera obtained from leprosy patients. Phytopharmacological examination of such sera, by Macht's methods, on the growth of *Lupinus albus*, has revealed for the first time that such blood sera contain a very powerful toxin, a toxin of even greater potency than that of pernicious anemia. This toxin is quite different from that of pernicious anemia in respect to the effects of ultra-violet rays. Whereas ultra-violet rays destroy the toxin of pernicious anemia, they do not affect, to any appreciable extent, the toxin of leprosy. A comparative study has been carried on by the author on the blood serum of leprosy, on the one hand, and two diseases which simulate it in certain respects, on the other. These diseases are tuberculosis and syphilis. The author wishes to announce in this place the results of these studies. A large series of experiments reveals that the sera of tuberculosis and syphilis are both even less toxic than normal blood serum, whereas the serum of leprosy exhibits an extreme toxicity. Thus, by means of a phytotoxic or phytopharmacological test, it has been our good fortune to contribute to some extent not only to the question of the etiology of leprosy but also to its differential diagnosis from syphilis and tuberculosis (Table I).

In view of the interesting findings obtained by the author in connection with the liver treatment on the blood of pernicious anemia, it was deemed of interest to study phytopharmacologically the effects of certain chemotherapeutic agents on the toxin of leprosy. All these studies were carried on in the author's laboratory on sera of leprosy patients sent to him through the courtesy of medical men in the field, to whom grateful acknowledgment is due. In this connection, the author wishes to make an announcement of certain very interesting findings. Studies were

TABLE I

COMPARATIVE TABLE OF BLOOD SERA

Kind of blood serum	Average of no. of cases	Phytotoxic index	Effect of ultra-violet irradiation
		Per cent.	
Normal human	100	72	no effect
Menstrual	50	51	more toxic
Pernicious anemia	48	44	detoxification
Pemphigus	18	54	slightly less toxic
Leprosy	22	47	not detoxified
Tuberculosis	27	78	no change
Syphilis	20	81	no change

made on leprosy sera with and without the admixture of dilute solutions of extracts of chaulmoogra oil or its esters, care being taken to make all conceivable controls and checks in regard to the effect of chaulmoogra oil and its esters alone on living plant protoplasm. It was found that chaulmoogra oil and its esters exerted a truly chemotherapeutic detoxifying effect on the toxin of leprosy, whatever it may be. The author is happy to announce, furthermore, that through the courtesy of Professor R. N. Chopra, of the School of Tropical Medicine, Calcutta, India, he examined a series of blood sera from both treated and untreated leprosy patients, and the findings in toxicity in these cases agreed with the laboratory findings in Baltimore. The untreated cases showed the greatest toxicity; the cases treated with chaulmoogra showed a definite decrease in the amount of toxin in their blood, thus clinically confirming the findings made in the author's laboratory. These studies, of course, are of a limited nature but it is hoped that they will lead to further work on the subject (Table II).

TABLE II

CHAULMOOGRA EXPERIMENTS

	Phytotoxic index
I. Leprosy Blood A (India)	Per cent.
1. Serum, 1 per cent., alone in Shive	49
2. Serum, 1 per cent., alone in Shive, irradiated with quartz lamp	45
3. Chaulmoogra ester alone in Shive	73
4. Serum, 1 per cent., plus chaulmoogra ester in Shive	71
II. Leprosy Blood B (America)	
1. Serum, 1 per cent., alone in Shive	58
2. Serum, 1 per cent., alone in Shive, irradiated with quartz lamp	58
3. Chaulmoogra oil alone in Shive	78
4. Serum, 1 per cent., plus chaulmoogra oil in Shive	80
III. Pernicious Anemia Blood	
1. Serum, 1 per cent., alone in Shive	25
2. Serum, 1 per cent., plus chaulmoogra ester in Shive	24
3. Serum, 1 per cent., irradiated with quartz lamp	60

STUDIES IN DERMATOLOGY

The author, in collaboration with Dr. Isaac Pels, a dermatologist, has been examining the phytopharmacological reactions on sera from all kinds of skin diseases, and such a routine study has led to the discovery of another toxin in connection with one more baffling disease, namely, the very grave condition known as pemphigus. Macht and Pels have succeeded in demonstrating the presence of a toxic substance in the blood, as well as in the contents of the bullae, of pemphigus patients, which is a contribution to the etiology of this hitherto entirely obscure and misunderstood pathological condition.[13] Studies on other dermatoses are in progress.

STUDIES ON ECLAMPSIA

Eclampsia is one of the gravest pathological complications of pregnancy and puerperium. Various theories concerning its causation have been advanced, the most popular being that which regards it as a toxemia. This theory, however, is not generally accepted because no toxin has hitherto been demonstrable in all the clinical and chemical studies of eclamptic patients that have been made. Even transfusions of eclamptic blood into other animals fail to produce the disease. A new conception regarding eclampsia, which differs from the old toxemia theory, has recently been advanced by Zanggemeister.[14] It was interesting, therefore, to inquire whether a toxin could be demonstrated in eclamptic blood by the newer phytopharmacological methods. Such an investigation has been undertaken by Macht and Losee, and it may be announced in this place that their findings corroborate the newer views concerning the etiology of eclampsia. No toxic substance in the blood serum of eclamptics could be detected by phytopharmacological methods.

ZOOGENIC *versus* PHYTOGENIC POISONS

The experimental investigations described above definitely reveal that certain poisons, or toxins, which could not hitherto be detected by chemical, physical or by zoophysiological and zoopharmacological methods, could be demonstrated by phytopharmacological

[13] Macht and Pels, *Arch. Dermatol. and Syphilol.*, 19: 640–647, 1929.

[14] Zanggemeister, "Die Lehre der Eklampsie," Leipzig, 1926.

means and, in general, it has been the experience of Macht and his coworkers that poisons produced by plants, or phytogenic poisons, are more toxic for animals than for plants, while poisons elaborated or produced by animals, or zoogenic poisons, are commonly much more toxic for living plant protoplasm than for living animal tissues. A series of investigations carried on by the writer tends to support this view. Macht has examined a large number of alkaloids, glucosides and other plant products known to be extremely poisonous for animals and found them to be comparatively very little toxic for living plants. On the other hand, he has made studies of various drugs and poisons of animal origin and found them to be extremely toxic for living plants. Thus it has been found that nicotine, atropine, morphine, aconitin, gelsemine, homatropine, lobelin, scopolamin and ouabain, all very powerful poisons for animals, are very little toxic indeed for living seedlings of *Lupinus albus*. On the other hand, studies made with the poison of toads, scorpions, spiders and other animals were found to be extremely toxic for such living seedlings.

Striking Examples

To illustrate the difference in phytopharmacological reactions between the poisons derived from the plant and animal kingdoms, the following may be appropriately cited. A comparative study of the active principle of the suprarenal gland, known as epinephrine, on the one hand, and the alkaloid ephedrine, in many respects resembling the action of epinephrine on animals, revealed that, whereas epinephrine was very toxic for living plant protoplasm, ephedrine, or "vegetable epinephrine," was practically non-toxic for it.[15] The drug cantharidin, obtained from the Spanish beetle and used to some extent in medicine, was compared with a most powerful and deadly poison known as ricin, obtained from the shells of the castor-oil seed. It was found that ricin was not at all toxic for growth of *Lupinus albus* in Shive solution, whereas cantharidin, in concentration of 1:50,000, killed it.[16]

Applying phytopharmacological methods to the study of bee virus and extracts of red ants, the author succeeded in demonstrating conclusively that these substances are not irritant by virtue of their content in formic acid but that they contain small quantities of unknown animal toxins.

[15] Macht, *Arch. f. exper. Path. u. Pharmacol.*, 143: 329–336, 1929.

[16] Macht, *Proc. Soc. Exper. Biol. and Med.*, 25: 592–593, 1928.

Snake Venoms

One of the most interesting studies carried on at present by the author is that on snake venoms. The venoms of cobra, rattlesnake, moccasin and other reptiles are being investigated. These poisons, of course, are well known to be extremely deadly for man and other animals. A phytopharmacological study of such poisons, made by dissolving the dried venom in plant-physiological solutions and growing various plants in them, reveals that, if anything, snake venoms are even more poisonous for living plant protoplasm than for animal protoplasm. The author is developing a method of quantitatively expressing the toxicity of these venoms and purposes to use it in connection with the study of various antidotes and therapeutic agents employed for snake-bite.

Conclusion

Thus it will be seen that, starting with a purely abstract objective, namely, the comparative study of animal and plant protoplasm in respect to their reaction to various poisons and chemicals, a number of interesting discoveries have been made. These studies are of interest not only to the medical man but also to the biologist, including the botanist and zoologist, on the one hand, and to the chemist, pharmacologist and pharmacist, on the other. The same methods have also been employed by the author for comparative studies of all kinds of chemicals, such as organic mercurials, preparations of digitalis, etc., and have been found very useful. In view of the fact that in all such experimental observations, made not on one or two individuals but generally on ten living plants and sometimes more, an average reading of all the measurements is taken, the results obtained are certainly just as reliable as those gathered from animal test objects, if not more so.

The author is quite aware that the subject of phytopharmacology is only in its infancy, that many additions and modifications in regard to the methods of study and even some of the data adduced will certainly be made, but this is true of all scientific investigation. The results reaped already from his studies on menotoxin, pernicious anemia, leprosy, pemphigus, eclampsia, snake venoms and various animal poisons, however, have fully demonstrated a definite value for this new line of work and, if these discoveries will serve to direct the attention for other investigators to the field, he will be more than grateful for having had the privilege of contributing what little lay in his power to the advancement of experimental science and benefit of humanity. It may be well to conclude with a quotation from an ancient sage:

'Tis not incumbent on thee to complete the whole,
Yet art thou not absolved from doing what thou canst.

OBITUARY

FREDERICK SLATE

FREDERICK SLATE, professor of physics emeritus, University of California, died at his home in Berkeley on February 26, at the age of seventy-eight years. He was born in London and came to the United States when twelve years of age. He took his bachelor's degree in 1871 at Brooklyn Polytechnic Institute, after which he accepted a position as civil engineer with the Northwestern Pacific Railroad. He entered the University of California as a graduate student in 1873, becoming graduate assistant in chemistry in 1875 and instructor in chemistry in 1876. After two years of study in Germany, he returned to the University of California in 1879 as superintendent of the physics laboratory. On the death of Professor John LeConte in 1891 he became head of the department of physics, which position he held until his retirement in 1918. The University of California conferred on him the degree of LL.D. in 1925.

Professor Slate was active in gaining recognition for scientific courses in the university curriculum and the establishment of a College of Natural Science. He was dean of this college from 1896 to 1909. He took an active part, not only in the early development of the university, but also in the establishment and early growth of the high school system throughout the state. His influence was always on the side of solid foundations and high standards. His publications were mainly in the field of mechanics. In the class room he possessed to an unusual degree the power of clear exposition and the ability to inspire his students with the spirit of work. His teaching was marked by strong individuality and by the demand for exactness in thought and expression. He was a potent inspiration to many. He was a man of great sincerity, truthfulness and loyalty.

ELMER E. HALL

UNIVERSITY OF CALIFORNIA

CHRISTINE LADD-FRANKLIN

THE career of Dr. Christine Ladd-Franklin, who died on March 5, 1930, at the age of eighty-two years, was remarkable in several ways. It was remarkable for brilliancy of achievement. Her theory of color vision, whether or not it shall be the final word on the subject, has certainly done excellent service by holding together the most important facts, and by relating psychology, physiology and photochemistry, along with an evolutionary conception of the development of the color sense. This theory arose out of her study of the theories of Helmholtz and Hering during her work in Germany in 1891–92. She pointed out defects in each of these rival theories and showed how the merits of each could be combined into a single theory. Her work in symbolic logic, perhaps even more brilliant than that in color theory, dates from even further back, from her early days at the Johns Hopkins in 1878–82. Besides these major achievements there were several others of interest, especially her discovery with König in 1895 of the "normal night-blindness of the fovea," and her resuscitation of the "blue arcs" in 1926, and making something of theoretical interest out of this phenomenon.

One of the most remarkable features of her career was her vitality to the last. Her output of publications, which declined from the time when she was about fifty-five, mounted again when she was seventy-five. She maintained to the last her keen interest in all the new developments in her chosen lines. Little of importance escaped her notice, and her judgment and keen power of criticism remained as good as ever. Nor was there any diminution in her zeal for promoting the views which she had espoused.

One should not omit to mention the strong feminist element in her motivation. She was proud of having been a pioneer in university study by women at a time when special private arrangements had to be made in order to allow her the privilege of a university connection, and she was keenly interested in every fresh advance made by women in scientific production. To those who saw her at close range, with her keen logical mind and her cheerful aggressiveness, she certainly appeared a remarkable woman.

R. S. WOODWORTH

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RECENT DEATHS

PROFESSOR STEPHEN A. FORBES, since 1917 chief of the Illinois State Natural History Survey, professor at the University of Illinois from 1884 to 1921, and dean of the college of science from 1888 to 1905, died on March 13 after a week's illness, at the age of eighty-five years. He had been continuously in the state's service for nearly sixty years.

THE death is reported of Dr. Thomas Rakestraw Baker, professor emeritus of natural science at Rollins College, Florida. He was ninety-three years old.

DR. HENRY CHAPMAN MERCER, anthropologist, archeologist, historian and founder of the Mercer Museum at Doylestown, Pennsylvania, connected with the Bucks County Historical Society, died on March 9 at the age of seventy-four years.

Nature reports the following deaths: Dr. G. G. Chisholm, formerly reader in geography in the University of Edinburgh and secretary of the Royal

Scottish Geographical Society from 1910 until 1925, on February 9, aged seventy-nine years; Mr. A. A. Campbell Swinton, F.R.S., known for his pioneer work on X-rays and radio communication, on February 19, aged sixty-six years; Dr. F. Arnall, head of the department of pure and applied chemistry at the Cardiff Technical College, whose interest was mainly in organic chemistry, on February 7, aged thirty-four years, and Professor Felix M. Exner, director of the Zentralanstalt für Meteorologie und Geodynamik and professor of terrestrial physics in the University of Vienna, who was an honorary member of the Royal Meteorological Society, on February 7, aged fifty-three years.

SCIENTIFIC EVENTS

THE FAUNA OF THE BRITISH EMPIRE

THE annual meeting of the Society for the Preservation of the Fauna of the Empire was held on March 4 at the offices of the Zoological Society, London. Lord Onslow, the president, was in the chair.

According to the London *Times*, the report of the executive committee stated that since the last report the acting secretary had made a visit to America solely with the object of enlisting the cooperation of American conservationists in the work of the society. An influential committee had been formed in New York, under the auspices of the Boone and Crockett Club, with Mr. Madison Grant as chairman, and it included Professor Henry Fairfield Osborn, Colonel Kermit Roosevelt, Mr. Childs Frick and Major F. R. Burnham. This committee had charged itself with the collection of a fund to promote efforts with which American conservationists are in sympathy, and it was believed that the society would thereby receive substantial help. The Wild Life Protection Society of America had also, through Dr. Hornaday, generously made a contribution to their funds.

It was impressed on our secretary in America that it was expected that any help forthcoming from the citizens of the United States of America would be equaled by similar support from our own people. We are confident that this will be the case, but definite efforts are necessary, and we urge our members not to fail us in this respect. The need is urgent. To meet changing conditions we must extend the sphere of our activities. The character of our endeavor during the next ten years will do much to decide the fate of the wild life of the world. This may sound ambitious, but as some 70 per cent. of the larger mammals of the world are in this empire, the responsibility resting on those who claim its citizenship is not to be disregarded. This society, based as it is in the capital city of the empire, and with its twenty-five years' record of achievements behind, can, it is submitted, claim to be the correlating focus of wild life conservation for the dominions, colonies and dependencies which all go to make up the empire of to-day.

Lord Onslow reported that the society wished to carry preservation much farther than it had ever been carried before. That must be done if wild animals were to be preserved in their wild state. As time proceeded, it became more necessary to stabilize reserves by establishing them as absolute sanctuaries. That had been done in America and other countries, and it was by the establishment of national parks in the future that this problem would probably be solved.

He was glad to say that a debate which took place in the House of Lords elicited a satisfactory reply from Lord Passfield, both as to the general policy of the government in regard to game preservation, and to an assurance that the monstrous practice of slaughtering game from motor-cars would be severely dealt with.

A CENTURY OF PROGRESS IN CHEMISTRY

A GROUP of chemists representing leading laboratories in educational institutions and industrial organizations throughout the country has recommended plans for a chemistry exhibit at the Chicago World's Fair in 1933, it is announced by Maurice Holland, director of the National Research Council Science Advisory Committee to the fair.

The group, under the chairmanship of Dr. Arthur D. Little, of Cambridge, Massachusetts, has been collaborating with the Science Advisory Committee in the development of plans for exhibits by all the sciences at the Chicago Fair which is to be held to celebrate the hundred years of progress made since Chicago became a city in 1833.

The chemistry exhibit will include representations of the laboratories, respectively, of an alchemist of the Middle Ages, of some chemist of 1833, and the modern laboratory of 1933. It is suggested that the laboratory of 1833 be the replica of the laboratory of some famous chemist of a century ago. The modern laboratory would be completely equipped for analyses, control work and research.

To illustrate the comprehensive contribution of chemistry to our daily modern life, it is proposed that a typical living room of the 1833 period be exactly reproduced alongside a living room of to-day by which the differences in living conditions in the two periods would be demonstrated. A descriptive pamphlet telling the story of the change and the part played by chemistry would be distributed. A kitchen of 1833 and one of 1933 are also proposed.

The report recommends that the chemistry exhibits should stress fundamental discoveries and their relation to industry and human welfare; that where desirable discoveries made prior to 1833 should be included; that exhibits should be developed along chronological lines, though not necessarily by distinct periods of time.

It is further suggested that wherever possible the exhibit should include and be developed from something with which the public is familiar, and that the object from which such development should be shown might be either a raw material such as wood or coal, or a final product like an automobile or artificial silk.

The report recommends that one major exhibit be devoted to the study and romance of carbon; that photochemistry should be given emphasis in a special exhibit, stressing its chemical and agricultural aspects as distinct from photography, and that there should be an exhibit illustrating atomic and molecular structure.

It was also suggested in the report that framed portraits of chemists who have made outstanding contributions to the science since 1833 be freely used. A further recommendation provides for a monument emblematic of chemistry, which would embody movement and color and which could be seen 300 feet away.

The chemistry committee in addition to Dr. Little consists of Professor Roger Adams, University of Illinois, Urbana; Professor Wilder D. Bancroft, Ithaca, N. Y.; Professor Marston T. Bogert, Columbia University; Professor R. T. Haslam, Belmont, Massachusetts; Professor Arthur J. Hill, Yale University; Dr. Harrison E. Howe, editor of *Industrial and Engineering Chemistry*, Washington, D. C.; Professor Frederick G. Keyes, Massachusetts Institute of Technology; Professor Arthur B. Lamb, Harvard University; Dr. Irving Langmuir, General Electric Company; Dr. C. E. K. Mees, Eastman Kodak Company; B. C. Mougey, General Motors Corporation; Professor Lyman C. Newell, Boston University; Professor James F. Norris, Massachusetts Institute of Technology; James A. Rafferty, Carbide and Carbon Chemicals Company; Dr. Allen Rogers, Pratt Institute; Charles F. Roth, New York; Professor H. C. Sherman, Columbia University; Dr. C. M. A. Stine, E. I. du Pont de Nemours & Company, and Dr. Willis R. Whitney, General Electric Company.

The Science Advisory Committee is headed by Dr. Frank B. Jewett, president of the Bell Telephone Laboratories, New York City.

THE GEOLOGICAL SOCIETY OF AMERICA

THE forty-first annual meeting of the Geological Society of America and its affiliated societies, the Paleontological Society and the Mineralogical Society of America, was held at the Wardman Park Hotel, Washington, D. C., December 26, 27 and 28, 1929, under the auspices of the Geological Society of Washington.

The meeting was one of the largest in the history of the society, 583 persons being registered. Eighty-five scientific papers were presented before the Geological Society, and the programs of the affiliated societies were also crowded. The address of the retiring president, Dr. Heinrich Ries, on "Some Problems of the Non-metallics," was delivered the evening of December 26, in the auditorium of the National Museum, and was followed by a smoker tendered by the Washington hosts. The annual dinner was held at the Wardman Park Hotel on the 27th.

The officers of the society for the year 1930 are:

President, R. A. F. Penrose, Jr.
Vice-presidents, Nelson H. Darton, Florence Bascom, Herbert E. Merwin, W. H. Twenhofel
Secretary, Charles P. Berkey
Treasurer, Edward B. Mathews
Editor, Joseph Stanley-Brown
Councilors, George R. Mansfield, William E. Wrather, Herdman F. Cleland, Elwood S. Moore, W. C. Mendenhall, W. J. Mead
Representative of the Cordilleran Section, Eliot Blackwelder

IN HONOR OF DR. WELCH

AN international birthday celebration is being planned for the "dean of American medicine," Dr. William Henry Welch, of the Johns Hopkins University, who will be eighty years old on April 8. Simultaneous ceremonies in honor of Dr. Welch will be held on that day in London, Paris, Berlin, Leipzig, Tokio and Pekin, as well as in Baltimore, Cincinnati, New Haven, New York and Washington, D. C., according to the plans announced by the executive committee in charge of the arrangements, of which Dr. Simon Flexner is chairman.

At Washington, the focal point of the celebration, President Hoover will speak on a program in Memorial Continental Hall, beginning at noon, which will be heard over a national hook-up of the National Broadcasting Company. It is expected that the program will also be broadcast by short wave and that it may be heard by those participating in the simultaneous ceremonies in foreign countries.

A unique feature of the celebration has been arranged through the cooperation of Alfred Hutty, the etcher, of Charleston, South Carolina, who was commissioned to make a dry-point portrait of Dr. Welch. The first print from the plate will be given to Dr. Welch at the Washington ceremonies, and there will be simultaneous presentation of other prints from the same etching to more than forty institutions in this country and abroad, with which Dr. Welch has been connected as student, teacher or adviser.

President Livingston Farrand, of Cornell University, has accepted the invitation of the committee to deliver the opening address at the Washington ceremonies, which will be witnessed by an invited audience of eminent scientists and other distinguished men and women.

The executive committee in charge of the celebration consists of Dr. Flexner, chairman; John A. Kingsbury, 49 Wall Street, secretary; Dr. William H. Howell and Dr. William G. MacCallum, of Baltimore; Dr. William T. Councilman and Dr. Harvey Cushing, of Boston; Abraham Flexner, Homer Folks, John D. Rockefeller, Jr., and Wickliffe Rose, of New York; Dr. Eugene L. Opie, of Philadelphia, and Vernon Kellogg and Senator Frederic C. Walcott, of Washington. Professor Albert A. Michelson, of the University of Chicago, has accepted the chairmanship of the general committee, which is now being formed.

"America owes more to Dr. Welch than can ever be told in any tributes," according to the statement issued by the committee. "Half a century ago he began his leadership in modernizing American medicine. Through the reforms which he has instituted in medical study, through his researches, the many hundreds of doctors trained by him, and the vital public health measures which he has inspired, it is literally true that millions have benefited from his contribution."

THE NINTH PLANET

ANNOUNCEMENT was made on March 13 by Dr. V. M. Slipher, director of the Lowell Observatory at Flagstaff, Arizona, of the discovery of a celestial body whose rate and path among the stars indicate a new planetary member of the solar family beyond the outermost known planet, Neptune.

Twenty-five years ago Dr. Percival Lowell, who founded Lowell Observatory, began a mathematical investigation for a planet beyond Neptune. The probability of locating such a body, however, was difficult and involved enormous and intricate computations.

In 1914 he announced as the result of his calculations the possibility and distance of the predicted body in a large memoir, a Lowell Observatory publication.

The search of the skies directed by Dr. Lowell's theoretical work was begun by photography in 1905 and has been continued to the present time. The use was made of the best available instruments, the search covering that band of the skies in which the known planet traveled.

Early last year, the Lawrence-Lowell telescope, a highly efficient special instrument for the search, was put in operation. Some weeks ago (January 21) Mr. C. W. Tombaugh detected an object on a plate made with the telescope, which has since been followed carefully.

It has been observed photographically with the large Lowell reflector by C. O. Lampland and it has been observed visually with the larger refractor by the various members of the staff.

All observations indicate the object to be the one which Lowell saw mathematically.

SCIENTIFIC NOTES AND NEWS

THE annual stated meeting of the National Academy of Sciences will be held in Washington on April 28, 29 and 30.

THE regular spring meeting of the executive committee of the American Association for the Advancement of Science will occur at Washington on Sunday, April 27. Memoranda concerning business to come before the committee at that meeting should be received at the permanent secretary's office in the Smithsonian Institution Building by April 21.

PROFESSOR KARL TAYLOR COMPTON, now head of the department of physics of Princeton University, has been elected president of the Massachusetts Institute of Technology. Dr. Compton will take office next July, when Dr. Stratton will become chairman of the executive committee and of the corporation, a newly created position in which he will share the responsibilities of administration.

THE American Institute of Chemists has awarded its medal "for noteworthy and outstanding service to the science of chemistry and the profession of chemist in America" to Mr. George Eastman, of the Eastman Kodak Company.

THE Society of Arts and Sciences has awarded its annual gold medal to Dr. J. McKeen Cattell, psychologist of New York, and to Professor Gilbert N. Lewis, physical chemist of the University of California, for outstanding accomplishment in the field of science. Mr. Walter Russell, president of the society, announces that the formal presentation will be made at a dinner at the Hotel Biltmore on April 17. Last year Professor A. A. Michelson and Professor R. A. Millikan received the medals. Mr. Thomas A. Edison was the recipient in 1928.

THE Medical Faculty of the University of Freiburg on March 11 conferred an honorary doctorate on Dr. Herbert M. Evans, professor of anatomy in the University of California, in recognition "of his conspicu-

ous anatomical and biological discoveries, especially in the sphere of vitamin research, both of which are scientific and of world interest."

DR. FLOYD K. RICHTMYER, professor of physics at Cornell University, will receive the Louis E. Levy Medal awarded by the committee of science and arts of the Franklin Institute of Philadelphia, at the annual medal-day exercises to be held in the hall of the institute on May 21, for his paper on "Some Secondary Phenomena in X-ray Spectra," which appeared in the *Journal* of the institute for September, 1929.

DR. THOMAS DARLINGTON, of New York City, celebrated on March 12 the fiftieth anniversary of the beginning of his practice. In recognition of his service to the city as head of the Health Department from 1904 to 1910, he received from Dr. Shirley W. Wynne a gold badge bearing the title "Health Commissioner Emeritus."

BARON KOI FURUICHI, of Tokyo, has been elected an honorary member of the British Institution of Civil Engineers.

THE council of the Royal Society, at its meeting on February 20, as reported in *Nature,* has recommended for election into the society the following fifteen candidates: Herbert Stanley Allen, professor of natural philosophy, University of St. Andrews; Edward Battersby Bailey, professor of geology in the University of Glasgow; Frederick T. Brooks, university lecturer in botany, Cambridge; Paul Adrien Maurice Dirac, university lecturer in mathematics, Cambridge; Harold Ward Dudley, chemist, National Institute for Medical Research, Hampstead; Charles Alfred Edwards, principal and professor of metallurgy, University College, Swansea; Harry Eltringham, entomologist, Hope Collections, University of Oxford; Charles Edward Inglis, professor of mechanism and applied mechanics, University of Cambridge; Harold Spencer Jones, H.M. astronomer at the Cape of Good Hope; Eric Keightley Rideal, lecturer in physical chemistry, University of Cambridge; Robert Robison, assistant chemist, department of biochemistry, Lister Institute; John Stephenson, formerly lecturer in zoology, University of Edinburgh; George Paget Thomson, professor of natural philosophy, University of Aberdeen; Charles Todd, department of experimental pathology, National Institute for Medical Research; William Whiteman Carlton Topley, professor of bacteriology and immunology, London School of Hygiene and Tropical Medicine.

AT a meeting of the Geological Society of London, held on February 21, the following officers and new members of council were elected: *President,* Professor E. J. Garwood; *secretaries,* Mr. W. Campbell Smith and Professor W. T. Gordon; *foreign secretary,* Sir Arthur Smith Woodward; *treasurer,* Mr. F. N. Ashcroft; *new members of council,* Professor P. C. H. Boswell, Professor C. G. Cullis, Mr. J. F. N. Green, Sir Albert Ernest Kitson and Dr. Bernard Smith.

THE retirement is announced of Dr. Charles A. Wheeler, professor of mathematics at the Connecticut Agricultural College, after thirty-three years of service.

PROFESSOR RODNEY W. BABCOCK, head of the department of mathematics at De Pauw University, has been appointed dean of the division of general science at Kansas State Agricultural College, and will enter upon his new work on July 1. Dr. Babcock succeeds Dr. J. T. Willard, who is serving his forty-sixth year as a member of the faculty of the college and who will be assigned exclusively to the duties of vice-president.

DR. FREDERICK A. COLLER, professor of surgery, has been named director of the department of surgery in the medical school of the University of Michigan. The appointment took effect on the first of March. Dr. Coller, who succeeds Dr. Hugh Cabot, went to the university in 1920 as an assistant professor of surgery. In 1928 he was advanced to the standing of full professor. Dr. Cabot has been appointed senior consultant at the Mayo Clinic, Rochester, Minnesota, effective on June 1.

DR. THOMAS PARRAN, JR., assistant surgeon general in the U. S. Public Health Service, was appointed New York state commissioner of health on February 19. His appointment was confirmed by the Senate on March 5.

CYRIL AINSWORTH, formerly director of the Bureau of Industrial Standards of the Pennsylvania Department of Labor and Industry, has been appointed to direct the national safety code work of the American Standards Association, as announced by William J. Serrill, president of the association. Mr. Ainsworth will direct the work of the association in the establishment of national codes for the guidance of state and municipal governments, industrial organizations and insurance companies. The appointment to the American Standards Association staff has also been announced of John Wilson McNair, formerly of the standards department of the American Institute of Electrical Engineers, and Clarence Edward Darling, formerly of the staff of the American Society of Mechanical Engineers. These appointments are part of a program of extension of national industrial standardization activities. It is expected

that the association will have nearly $500,000 available for its work during the next three years.

DR. H. A. GLEASON, curator in the New York Botanical Garden, has been appointed associate editor of the new periodical, *Ecological Monographs,* to be published for the Ecological Society of America by Duke University. Publication will begin in 1931.

PROFESSOR ROBERT F. MCCRACKAN, of the department of chemistry of the Medical College of Virginia, has been appointed chairman of the committee on education by radio of the American Chemical Society. Dr. Otto J. Sieplein, of the University of Miami, and Dr. S. B. Arenson, of the University of Cincinnati, are the other members of the committee.

DR. AND MRS. N. L. BRITTON, who have been spending the winter in Porto Rico, where they are continuing their studies of its flora, are expecting to return to New York in April.

AFTER a year of travel in which he covered a distance estimated at 43,000 miles, Professor H. L. Clark, of the Harvard Museum of Comparative Zoology, has returned. He is preparing for classification a large collection of specimens of marine life.

DR. W. W. SKINNER, of the Bureau of Chemistry and Soils of the U. S. Department of Agriculture, accompanied by several chemists of the bureau, is visiting Minneapolis, Madison, Dubuque and Chillicothe, to investigate activities in the utilization of cornstalks, wheat straw, oat hulls, and other farm by-products. They plan to look further into the possibilities of combating the corn-borer by the commercial utilization of corn-stalks.

DR. LEE R. DICE, curator of the mammal division of the University of Michigan Museum, and William P. Harris, Jr., of Detroit, associate curator, will make their headquarters at the Desert Laboratory, in Tucson, Arizona, for the next two months, while they study wild life in the desert, particularly mice, rabbits and squirrels.

PROFESSOR HERBERT R. MOODY, director of the department of chemistry of the College of the City of New York, has received a leave of absence for the spring term. He will devote time to a study of chemical industries in Italy and southern France, and will also attend the annual meeting of the Society of Chemical Industry in Birmingham, England, in July. While he is away Professor Reston Stevenson will act as director of the department.

DRS. T. M. LOWRY, of Cambridge University, and R. A. Morton, of the University of Liverpool, will be visiting professors on the faculty of chemistry of the Ohio State University during the coming summer quarter. Dr. Lowry will offer courses on optical activity in organic compounds and also on the physical basis (mainly spectroscopic) for chemical theory. Dr. Morton will offer a series of lectures in photochemistry. Professor H. A. Wilson, of the Rice Institute, and Professor W. W. Watson, of Yale University, will also be in residence during the summer quarter. Professor Wilson will lecture on the conduction of electricity through gases and the application of thermodynamics to chemical problems, and Professor Watson on molecular spectra and related topics.

DR. KIRTLEY F. MATHER, professor of geology at Harvard University, began on March 4 a course of eight illustrated lectures at the Lowell Institute on "The Geologic History of Mankind."

DR. WILLIAM F. FOSHAG, of the Smithsonian Institution and the U. S. National Museum, has been giving a series of twelve lectures at Princeton University at the invitation of the department of geology, on the "Ore Deposits of Mexico." One lecture was given on the "Saline Deposits of the Western United States."

PROFESSOR ALEXANDER SILVERMAN, head of the department of chemistry of the University of Pittsburgh, gave an address at the University of Wisconsin on March 19, on "Educational and Research Institutions of Europe." He addressed the Madison section of the American Chemical Society on the evening of March 19, on "Recent Developments in Glass Manufacture." Professor Silverman also addressed the Milwaukee Section of the American Chemical Society on March 21.

THE students of physics in the several collegiate institutions in and about Philadelphia held a second joint meeting for the discussion of "Opportunities in Physics" on February 18, in the hall of the Franklin Institute. The speakers were Dr. Horatio B. Williams, of the College of Physicians and Surgeons, New York City, who spoke on "Physics and Medicine"; and Dr. C. C. Kiess, of the Bureau of Standards, who spoke on "Physics and the Bureau of Standards."

AMONG recent speakers to the Gamma Chapter of Sigma Pi Sigma, honorary physics fraternity, at the Pennsylvania State College have been the following: Dr. Frank C. Whitmore, dean of the school of chemistry and physics at the Pennsylvania State College, who spoke on "Habits of the Atoms"; Mr. William F. Ruder, of the General Electric Company at Schenectady, who discussed "The Effect of Physical Condition on Magnetic Properties," and Dr. R. C. Gibbs, of

Cornell University, who presented an illustrated lecture on "Some Recent Advances in Atomic Spectra." Professor F. K. Richtmyer, of Cornell University, will address the chapter on May 7, on "X-ray Satellites."

DR. W. F. G. SWANN, director of the Bartol Research Foundation of the Franklin Institute, will present one of the principal addresses at the Eugene meeting of the Pacific Division of the American Association which meets from June 18 to 21. The subject of his address will be "Philosophic Concepts in Modern Physics."

THE Wisconsin Academy of Sciences, Arts and Letters, the Wisconsin Archeological Society, and the Midwest Museums Conference will meet jointly at the University of Wisconsin on April 11 and 12.

ON March 8 the Akron Society of Applied Physics and the Central Ohio Physics Club held a joint meeting to which physicists and those interested in physics in the industries and colleges of the state were invited. The program included visits to the laboratories of the B. F. Goodrich Rubber Company and to the Goodyear-Zeppelin Corporation. At a dinner in the University Club the speakers were: Dr. Dayton C. Miller, professor of experimental physics, Case School of Applied Science, "New Evidences of Motion of the Solar System"; Dr. Ellis L. Manning, Research Laboratories, General Electric Company, "A Study of Physics in Industry—Vacuum Tube Research"; Dr. Alpheus W. Smith (represented by Dr. C. W. Jarvis), professor of physics and chairman of the department, the Ohio State University, and Dr. L. H. Thomas, visiting professor at the Ohio State University, from Trinity College, Cambridge University. Action was taken to formulate some sort of organization plans for those interested in physics in the state of Ohio so that this type of program and joint meetings may be a recurrent affair.

HARVARD UNIVERSITY eventually will receive $50,000 for an endowed laboratory for applied practical hydraulics, under the terms of the will of the late Clemens Herschel, the hydraulic engineer. Mr. Herschel bequeathed to the Boston Society of Civil Engineers and the Boston Public Library the right to such of his books as they desire. He also provided that $1,000 each shall go to the Boston Society and to the Harvard Engineering School to be invested to provide prizes for studies in engineering.

THE Carnegie Corporation has recently made a grant of $7,500 to the Y. M. C. A. College Laboratory for Physiologic Research in Physical Education of Chicago. The funds were granted to further researches in the physiology of exercise now in progress.

THE Committee on Scientific Research of the American Medical Association has made a grant of $1,200 to the Medical College of Virginia, Richmond, for the study of lung involvement in human ascariasis. This investigative work will be directed by Dr. F. J. Wampler, professor of preventive medicine, and Dr. Lee E. Sutton, assistant professor of pediatrics. Rather extensive field work as a part of the proposed study will be undertaken during the coming summer.

DR. WILLARD ROUSE JILLSON, state geologist of Kentucky, announces an appropriation by the General Assembly of Kentucky of $100,000 annually during the next biennium beginning as of April 1, 1930, for cooperative topographic mapping in Kentucky. The state appropriation has been matched in the same amount by the topographic branch of the U. S. Geological Survey insuring an expenditure for the surveying of standard topographical sheets in Kentucky during the next two years of $400,000. About 70 per cent. of the 40,598 square miles in Kentucky is now topographically surveyed, and it is hoped to complete the state within about three years.

AN Institute for Fisheries Research has been established at the University of Michigan. It will be responsible for conducting the investigations in economic ichthyology called for by the Michigan Department of Conservation, which is subsidizing the institute. In investigations on the commercial fishes and fisheries of the Great Lakes, the Department of Conservation will continue to cooperate with the Great Lakes Laboratory of the United States Bureau of Fisheries, which like the new institute, has headquarters in the Museum of Zoology of the University of Michigan. The institute is planning to add a number of men to its staff, and expects to offer a number of fellowships in fisheries research.

THE Home for Incurables, the Bronx, New York City, will receive a bequest of nearly $900,000, according to the terms of the will of George W. Ellis, a real estate lawyer, who died on February 8. The State of Vermont will receive $100,000, providing that the University of Vermont, Middlebury College, of which Mr. Ellis was a graduate, and Norwich University are merged into one institution under state control. "This institution should eliminate all needless duplications and waste," wrote Mr. Ellis in his will, "and, regardless of creed or birth or wealth or social position, shall aim to educate and train morally, mentally and physically those expecting to retain a residence or occupation or property interest in the State of Vermont." If this provision is not carried out, the $100,000 bequest is to revert to Columbia University "for a like educational purpose."

TENNESSEE is the first state selected by the Commonwealth Fund for carrying out its new rural health project in cooperation with health departments in two or three states. Gibson county in the western part of the state has already been chosen as one of the two counties in which field work will be developed. In accordance with the general plan, Vanderbilt University is to receive a grant for developing the teaching of preventive medicine and providing opportunities for graduate study by rural physicians, and the University of Tennessee will be given funds to organize training courses for teachers in health education.

THE Kentucky Legislature has passed over Governor Sampson's veto the bill providing for acceptance of the offer made by Former Senator T. Coleman du Pont, of Delaware, to provide $230,000 for the acquisition for state park purposes of an area of approximately 2,200 acres at Cumberland Falls.

Museum News reports that an official party representing the Department of Interior and interested organizations made an inspection, lasting from February 11 to 15, of the site of the proposed Tropic Everglades National Park in the Cape Sable district of Florida. This party included Dr. Hermon C. Bumpus, of the American Association of Museums; Dr. T. Gilbert Pearson, of the Audubon Society of America, and E. K. Burlew, personal representative of the Secretary of the Interior, as well as Director Horace M. Albright, and other officials of the National Park Service. The inspection was made under congressional authority. The itinerary included a cruise over the territory in a blimp, a trip by water down the Florida Keys and through Florida Bay and the upper reaches of White Water Bay, and an automobile tour out the Tamiami trial from Miami. The findings of the investigators have not yet been made public.

THE National Tuberculosis Association announces a limited number of fellowships in social research as related to tuberculosis, open to graduate students who have had special training in statistics, social science or public health. Preference will be given to candidates who are interested in pursuing research in public health after the completion of this fellowship. Researches on topics selected by the National Tuberculosis Association will be conducted in collaboration with colleges and universities, and each study will be under qualified academic leadership. Academic credit may be allowed for this research according to arrangements with the individual universities under whose supervision they are undertaken. Each fellow will be required to submit a written report at the completion of his fellowship grant, and the text of that report shall remain the property of the National Tuberculosis Association. Candidates will be considered not alone on academic standing, but on experience and general fitness for research work. The fellowship grants will date from the beginning of the academic year in the fall of 1930. They are for a twelve-month period, and the fellowship grant amounts to $1,500 for that period with a month's leave for vacation. Interested candidates should write to Jessamine S. Whitney, Statistician, National Tuberculosis Association, 370 Seventh Avenue, New York City, for further information.

DISCUSSION

THE ECLIPSE OF APRIL 28

THE brief duration and the narrow path of totality of this eclipse, visible in California, make the question of the accuracy of the predictions of some interest to observers. Perhaps a brief statement of the problem, free from technical terms, may be of value.

The errors of the prediction arise mainly from two sources, namely, those due to lack of full knowledge of the motions of the Earth, Moon and Sun, and those due to the topography of the Moon's limb. Owing to a variety of causes, the former have been much diminished during the past six years. There is still some uncertainty due to the changing rate of rotation of the Earth, but this affects the position of the path less than it does the time at which totality occurs. In the coming eclipse the position is of importance because a small error may cause the observer to miss totality altogether. In a discussion just published in No. 934 of the *Astronomical Journal,* Dr. D. Brouwer has used all the latest available information, and we estimate that the uncertainty, due to these causes, of the position of the path given by him in a direction perpendicular to the path is less than a quarter of a mile.

The uncertainty due to the topography of the Moon's limb is larger than this. We have plenty of evidence of valleys on the limb a mile or more deep, and of mountains even higher than this amount, but our knowledge of their exact position at the time of the eclipse is not sufficient to enable us to use it for prediction. A valley a mile deep at either of the positions where the grazing edges of the Sun and Moon give the limits of totality on the Earth may alter the edge of the path on the Earth by a like amount. In the present case, where the predicted width of the path is considerably less than a mile, it may result in a complete absence of totality anywhere. Professional observers are aware of these facts, and have made up their programs accordingly. Certain classes of ob-

servation can be usefully undertaken with these conditions in view.

ERNEST W. BROWN

YALE UNIVERSITY

WAS MAGENDIE THE FIRST STUDENT OF VITAMINS?

FEW investigators have had the rare fortune to make two classical discoveries in one series of investigations. To Magendie must be ascribed such a feat. In a report entitled the "Nutritive Properties of Substances which Contain Nitrogen," which was published in 1816,[1] Magendie showed that dogs can not live upon fats, sugar and water but must have some form of nitrogen in their food. The importance of this discovery was such that it seems to have eclipsed the remainder of the report.

Magendie fed his dogs upon a diet of sugar and water. They lost weight steadily. The remainder of the experiment can not be expressed more concisely than in the author's own words:

> In the third week his [the dog's] thinness increased, his strength diminished, the animal lost his liveliness, his appetite was not so keen. At this same period a small ulceration was developing in the center of the transparent cornea, first on one eye and then on the other, it increased rapidly and at the end of a few days was more than a "ligne" [two millimeters] in diameter. Its depth increased in the same proportion; soon the cornea was entirely pierced and the fluid of the eye was flowing out. This singular phenomenon was accompanied by an abundant secretion of the glands of the eyelids.

This animal died. Autopsy showed little other than the effects of inanition. Magendie was a careful investigator. The experiment was repeated two times with identical results. Has any modern investigator presented a clearer picture of xerophthalmia?

In order to test the nutritive value of fats, a diet of olive oil and water was fed another dog. This animal died but showed no xerophthalmia. Another dog fed butter fat and water developed xerophthalmia in one eye! These results are of special interest in the light of modern work, since they are the reverse of modern experiments.

Magendie seems to have realized that he was dealing with a dietary deficiency, since he found normal chyle in his animals at the time of autopsy. His own statement is that "it is thus evident that if these diverse substances [fat, sugar, gum and water] do not nourish, we should not attribute this to the fact that they are not digested."

Magendie noted the marked changes in the urine, feces and bile that resulted from a diet lacking protein and suggested: "Can we not reasonably presume, after the experiments which I have reported, that by diminishing the nitrogenous substances in food we diminish the proportions of materials in the urine which give rise to gall-stones?"

Not only did Magendie record the production of xerophthalmia in animals but he recognized the analogous conditions in man as a result of a restricted diet. He reported this as follows:

> A very interesting experiment has recently been done by an English doctor named Stark. This doctor wishing to estimate the nutritive properties of sugar lived on it exclusively for about one month, but he was then obliged to give up this régime. He had become very feeble and bloated. In his sight appeared livid red spots which seemed to announce the approach of an ulcer. He died a short time after his experiment and the people who knew him thought that he might have been the victim of it.

Magendie closes this classical report with, "I wish that physicians would be inclined to make trials of this kind. Physiology, animal chemistry and medicine can gain from it."

Must Magendie be termed the father of the vitamin hypothesis, as well as the discoverer of the need for protein in the diet?

C. M. MCCAY

ANIMAL NUTRITION LABORATORY,
CORNELL UNIVERSITY

[1] *Annales de Chimie et de Physique*, 1 series, 3: 66, 1816.

DROSOPHILA ONCE MORE

IN a recent number of SCIENCE[1] Dr. Bessie B. League publishes a summary of her observations on the reduction divisions in *Drosophila melanogaster*. It is gratifying to find some one on this side of the Atlantic who will admit that the phenomena accompanying the reduction division in this extremely variable species are abnormal. I have found genetical and cytological colleagues most determinedly of the opinion (not, however, expressed by publication) that the meiotic divisions of this species are quite normal, and so much so that it had not been worth while to figure them. I have had to point out that one so presumably well acquainted with the genetics and cytology of Drosophila as Professor Morgan admitted so late as 1925 that little or nothing was known of the reduction division in this species. I find great diversity of explanations offered by genetical colleagues of the peculiar situation which exists in regard to our knowledge of the reduction division in this species. I have already pointed out that although sex chromosomes in practically every other case among animals and plants are recognized and described in the reduction division, in *D. melanogaster*, in striking con-

[1] SCIENCE, 71: 99, January 24, 1930.

trast, in spite of the enormous amount of work on the species, no adequate account of the reduction division has ever been published until very recently. This in itself is a surprising anomaly which the Drosophilists owe it to themselves to explain.

Dr. League's observations, interestingly enough, correspond closely with those recently published by Zuitin in the Genetical Bureau of the University of Leningrad.[2] The article is entitled in the English summary which accompanies the Russian text, "On the Peculiarities of Spermatogenesis in *Drosophila melanogaster*." This author's account confirms the observations made by the present author, and he reaches the conclusion that *D. melanogaster* is an interspecific hybrid, on the basis of its abnormal cytology. The article is accompanied by a number of figures, which in some cases closely parallel those published by the writer in 1925.

It is thus clear, on the evidence of three independent observers, that there are abnormalities connected with the reduction division of *D. melanogaster*. It seems obvious that it is impossible to explain these abnormalities by reference to *D. melanogaster* alone. Obviously the general situation must figure largely in connection with the whole discussion. We know that in the case of many hybrids there are lagging chromosomes and chromosomes extruding into the protoplasm, such as are not found in the parent species of these hybrids. This is particularly true of hybrid plants, because our information in regard to these is very much fuller than it is on the zoological side. The cytological peculiarities of reduction division of *D. melanogaster* exactly parallel those found in hybrid plants. Dr. League explains the lagging chromosomes as resulting from the fragmentation of plasmosomes. This explanation is very difficult to accept in view of the fact that quite normal plasmosomes have been described both by Stevens and the present author.

A very strong point in connection with this discussion is the fact that synapsis has been observed in the reduction division, and has been figured in two of the articles published by the present writer. The synaptic mates which, as is well known, appear in diakinesis and the metaphase of the reduction division in plants and animals are distinctly present and distributed along the spindle in a way which is common for a number of known hybrids, and for some extremely variable species which are probably of hybrid origin. While it may be possible to explain small clumps of apparent chromosomes occurring on the spindle and in the protoplasm in *D. melanogaster* as of plasmosomal origin or even as mitichondria, the synaptic pairs of chromosomes can not possibly be interpreted in this way. Further, fragmentation of chromosomes, as well as ejection into the protoplasm, is not uncommon in hybrids. It is thus extremely probable, in the light of all the facts, that the material lagging on the spindle and ejected into the protoplasm in the reduction division of *D. melanogaster* is of the nature of chromatin, especially as no corresponding structures are present in the somatic divisions. This view gains a high degree of credibility from the fact that we have these fragments of chromatin only in the heterotypic and homotypic divisions, precisely as is the case in known hybrids. This appears to show clearly that they have a peculiar and characteristic relation to meiosis. They parallel, in fact, in their behavior, exactly the conditions found in many known hybrids in plants and animals. It is accordingly assumed that the reduction phenomena in *D. melanogaster*, viewed from the general standpoint of our present knowledge of the cytology of hybrids, indicate clearly the hybrid origin of that species.

If the whole situation is summarized for *D. melanogaster*, it is very apparent that it manifests in a striking degree the peculiarities of an interspecific hybrid. In the first place, the species is heteroploid, showing not only eight and twelve somatic chromosomes but also dysploid modifications of these. This is a recognized heterozygous feature. Further, not only within this much-investigated species does heteroploidy occur, but in others of its large and variable genus, in which the specific enumeration has already reached two hundred. This is a feature of resemblance to other large and mutable genera which manifest all the characteristic features of hybrid contamination. Further, *D. melanogaster* is characterized by a high degree of sterility, another known heterozygous feature. Finally, we have at last the reasonably complete account of the reduction division from three independent, remote sources, which all set in relief the abnormalities in meiosis, which are precisely those found in hybrids. It is true that Dr. League claims that the real chromosomes of the species under discussion behave as in normal species. That is frequently the case in hybrids. Moreover, they do not behave normally in the crucial stage of the metaphase, in which the synaptic mates are present. Further, all recent investigators admit the presence of lagging material on the spindle and in the cytoplasm, precisely as is the case in known hybrids. Zuitin and the present writer claim that this material is chromatin, or at least contains chromatin, while Dr. League maintains the difficult position that it is of plasmosomic origin. If it were, we would expect to find similar material present in the somatic divisions, which is not the case. This consideration apparently eliminates the interpretations of both Dr. League and Belar of the

[2] *Bulletin* of the Bureau of Genetics, Vol. 7, Leningrad, 1929.

lagging and ejected material. Moreover, true plasmosomes are present in the spermatocytes and spermatids of *D. melanogaster*. Finally, the conduct of the lagging and ejected material is precisely that found in many known hybrids.

It is thus clear that *D. melanogaster* has so many of the characteristics of a hybrid that only its duplication by the experimental crossing of recognized species could supply additional evidence in this direction. It is further indicated that it is time that we had surcease of speculations on the part of geneticists as such on the general problems of evolution and the origin of species. No amount of Mendelian moiling with the stabilized variants of a mutable species is likely to throw any permanently valuable light on the question of the origin of species. The real problem is clearly that of the origin of mutability in modern types of plants and animals, a matter upon which the conscientious and even contentious elaboration of the laws of Mendel throws no light whatever. Cytology and the experimental crossing of species are obviously destined to lead the way to new and fundamental advances in our knowledge of the origin of contemporary species of plants and animals.

IN a recent number of this journal[3] Professor Huettner passes some criticisms on my work on *Drosophila melanogaster*. These are largely expressions of the dis-esteem of himself and his group of Drosophilists for my work on this species and includes some quite erroneous statements in regard to my qualifications. As these matters are of little scientific interest, it will be well to refer to the only significant feature of his paper, namely, the question he raises as to the lagging of the chromosomes in the species under discussion. He commends strongly in this connection the use of Feulgen's reagent for the identification of the real chromosomes. Putting aside the question as to the extreme abnormality of the reduction division in *D. melanogaster* as in itself a suspicious circumstance, my critic is referred both to the article cited above and more particularly to a recent paper by Woskressensky and Scheremetjewa on spermatogenesis in *D. melanogaster* published in the *Zeitschrift für Zellforschung und mikroskopische Anatomie*. The latter authors use the method of Feulgen, and their figures show a very large amount of lagging in the chromosomes of the species as diagnosed by this method. Further, their account agrees with that of the present writer in respect to the multiplication in number of the chromosomes beyond that to be expected from the somatic conditions. The tide in regard to the interpretation of *D. melanogaster* has apparently definitely turned, as the newer literature has abandoned the contemptuous attitude which has been in general adopted by Drosophilists on this side of the Atlantic.

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[3] SCIENCE, 71: 241, February 28, 1930.

SCIENTIFIC NAMES IN ZOOLOGY

A VERY considerable number of interested zoologists believe that real progress is being made in zoological nomenclature. A sane use of the highly valuable principle of priority is being made, despite the unwillingness of an ultra-conservative group to adopt any change from the nomenclature it is accustomed to and therefore regards as the proper one, and despite the objections of another group now also being forced into a conservative position by the progress which is being made. This second group would insist on the resurrection of any available name which has priority, no matter how much inconvenience the change would introduce. The International Commission on Zoological Nomenclature is in general steering a median course, by using its plenary power very wisely in eliminating from the rule of priority those occasional names which would clearly create a really wide-spread confusion and instability, and in consistently refusing to pass favorably on cases in which the replacement of the established name affects only a few systematists or in which the desirability of the replacement is clearly debatable or in which the proposed change involves taxonomic judgment rather than the application of the rules.

Those of us who view the progress in nomenclature more or less optimistically are perturbed by the expression of views which can only serve to obstruct the advance which is being made. We feel that such an article as that of Professor Needham[1] is particularly reactionary. Since this account is well and boldly written and since a considerable weight of authority is carried by the author, the remarks of Professor Needham are meeting in some quarters with a reception which we believe to be unjustly favorable. We feel constrained to attempt a reply.

Needham's "smouldering impulse first burst into flame" over some modern names to which he objects. Of course not all names are of equal euphony or brevity, and it is possible to search out some which are flagrant examples of poor style. Zoologists alone are not responsible for such unpleasant productions. They blight the terminology of other sciences. Indeed, some words in common use are not simple.

The discussion of these long names of the day by Dr. Needham gives a very unfair picture. Many modern names are a joy to the zoologist and the classicist alike; many old names are badly constructed and long. But that two names of record length "are far worse than anything pre-Linnaean" is not the truth. The first name quoted belongs to a set which the In-

[1] SCIENCE, 71: 26–28, 1930.

ternational Commission has recently specifically outlawed, in Opinion 105 (published June 8, 1929, with preliminary notices in 1927)—a fact of which our critic was either ignorant or which he ignored. The second name is further elongated by Needham himself by the insertion into its middle of three more letters, thus carrying it so far from bad to worse that the senior author of the name may perhaps be happy that Needham added a letter to his name also. Some of the names which Needham holds up to ridicule apparently sounded and looked bad to him because of their Slavic form, and to this degree his course is surely unjustified.

The impression is given by Needham that such awkward, bestretched names are very general: "If any one thinks that such monstrosities in names are isolated cases let him read the four pages of generic names derived from personal names in Palmer's 'Index Generum Mammalium.'" But the very names Needham cites are notorious exceptions; in fact, they are the very ones I myself have quoted in objecting to such names. We therefore can not regard such abuses as being at all common. The International Commission has refused to validate a group of crustacean generic names of which the one quoted by Needham is an example. On account of the circumstances of the case, I think the commission was justified in this arbitrary action, the first of its kind ever taken. But to outlaw inelegant names except in most unusual conditions would be dangerous and unjust, and would discredit the commission out of the authority it now commands among systematists.

It is amusing to note that men like Raffinesque a century ago replaced names which appealed to them as too long (or too short), but for such an action to be advocated to-day is an unpleasant anachronism.

Needham's critical attack appeals to us as essentially destructive. He does, however, make some suggestions, of very unequal promise. The general proposal for the erection of a new body to pass on taxonomic as well as nomenclatorial evidence and so build up a "standard name list" strikes us as very unwise and dangerous. Such a body would certainly command at most very limited and ephemeral authority. Active systematic zoologists as a whole would certainly not tolerate an effort to subjugate individual scientific judgment to an ordained system of names.

We trust that Dr. Needham will not deny that scientific judgment does enter into systematic work. We trust that he knows that even supposedly very well-known species are often found to be complexes of two or more species, or to be wrongly classified. We trust that he appreciates that accurate species identification is essential to sound comparative work in almost any field of zoology. We trust that he realizes that the fine distinction of forms is especially conducive to new interpretations in such fields as ecology, zoogeography and economic zoology. To have a fixed system of names for others than systematists would make the advances of systematists unavailable to other zoologists and would inhibit broad cooperative progress in the whole field. The suggestion for a name-fixing body seems to us little better than the discarded scheme for numbering species.

The proposal to submit our problems of nomenclature to psychologists will certainly hold little interest. The proposal for botanists and zoologists to cooperate may be worth considering, when the botanists among themselves agree to one code of rules. The proposal for "members-at-large in the name-choosing body, to secure a measure of uniformity" expresses a poor appreciation of the function which an International Commission on Zoological Nomenclature should (and has) assumed. The proposal to have members of the name-choosing body serving only for the groups in which they are taxonomic specialists, to bring to bear a working knowledge of the group and of its literature and tradition, would involve a commission of unwieldy size and overlooks the fact that the present commission ordinarily does submit cases to specialists in the group involved. The specific "tradition" of one group ought not to bear on questions of nomenclatorial rules, which are rightly the primary concern of the International Commission. The proposal for "Some better method of obtaining the opinion of zoologists than the *viva voce* vote of the crowded sessions of an international congress" is accompanied by no mention of the fact that the secretary of the International Commission makes strong efforts to bring proposed cases before the general zoological public for its opinions.

That the existing system of zoological nomenclature is in many ways crude and in need of revision is undeniable. That scientific names are so confused, absurd or appallingly long as to dull the interest of the pupils of conscientious teachers is a very minor pedagogical objection, if true. That "good judgment, expert knowledge, human sympathy, hard labor and long patience" will be required "to find a way out" is granted, but we need not completely ignore the fact that some degree of these noble qualities has already been mixed with the almost fiendishly egotistical passion of systematists (as pictured to us), in building up the nomenclature and the rules which hold to-day.

Needham is of course not alone in longing for a simple system of nomenclature. Most of us do. How to obtain such an end is a common question. The

solution is obviously, as Needham now admits, not to be found in any such device as numbering species. The use of quadrinomials I also object to; it is confined to few authors and to few groups of animals, and is not recognized by the International Rules, to which Needham makes too little reference. The use of trinomials to designate incompletely differentiated forms most systematists believe to be sound and unavoidable, and when logically applied leaves the binomial specific name available for the use of those who do not need or those who do not care or those who are unable to split the subspecies of the species in question.

Thus the evil, or virtue, of subspecies "splitting" need not worry those who long for a simple nomenclature (even if the simplicity be artificial). The splitting of species, when sound, unavoidably alters the scientific name and can be ignored only through ignorance or arrogance. The splitting of genera into subgenera need not worry the worshipers of brevity, for there is no need for quoting the subgenus in the scientific name. But the splitting of genera into smaller genera does alter the name. So does the transfer of species from one genus to another. The Rules of Nomenclature have no primary application to such taxonomic changes. These changes are at the base of an ever-increasing proportion of the unfortunate shifting of names. Fewer and fewer alterations are due to the uncovering of overlooked available names or to alteration of the species interpretations.

It is becoming increasingly clear that these name changes, due to genus splitting or shifting, are the chief concern of those who long for a stable nomenclature. It is unfortunate that the changes in genus concept should alter the scientific name of an animal. The fault lies in the binomial system of nomenclature. This system confounds classification, which ought to be flexible, with nomenclature which should be fixed. A uninomial system of animal names would divorce classification from nomenclature and would presumably emphasize the fact that the species is the most natural and objective of all systematic groups. It would certainly shorten animal names. The uninomial system has been found workable in mineralogy, chemistry and astronomy, and would have many advantages in zoology.

I do not propose the present adoption of any uninomial system of zoological nomenclature. I do emphasize, however, the facts that the tendency to split has continued, despite occasional set-backs by lumpers, from the time of Linnaeus until the present; that in some groups the splitting of genera has gone so far as to produce a high percentage of monotypic genera; that for such groups there is a tendency, in conversation or in general works or in the frequent repetition of the name in technical papers, to allow the generic name to stand for the whole scientific name. We are to this degree now heading toward a uninomial nomenclature of animals. That this system will be gradually and eventually adopted I venture to predict. If the uninomial system is not accepted, or until it is, I see no hope for ever arriving at a really stable nomenclature. In the meantime we can devise ways of surviving without this stability.

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SEA-LEVEL CHANGE NEAR NEW YORK

IN *Bulletin* of the National Research Council, Number 70, just issued, there is an erroneous statement. On page 35, paragraph D, it is stated that "Tidal observations at Fort Hamilton extending over a period of 35 years indicate no appreciable change in sea-level at that point during the period of observations."

As a matter of fact, the probable change in sea-level at Fort Hamilton between 1893 and 1927 is at the average rate of a rise of one foot in 214 years (by the least square method 0.0047 feet a year $\pm$ 0.06). Though the probable error of this result is great, it is more likely to be at the rate of 0.6 feet per century (.006 feet per year) as suggested by J. R. Freeman than to be with "no appreciable change."

Curiously, taking the last twenty-five years, from 1903 to 1927 inclusive, the rate would be .0055 feet a year.

The whole question deserves further consideration which we hope it will receive. For instance, M. R. Campbell's suggestion that meanders in streams flowing essentially at and below tide level are indicative of drowning, Bull. G. S. A. (1927) pp. 537–555, has a bearing.

ALFRED C. LANE,
WILLIAM FITCH CHENEY, JR.

ASTRONOMY IN SOUTH AFRICA

THE paragraph quoted from *Science Service* in the issue of December 20, 1929, headed "Astronomy in South Africa," contains several inaccurate statements.

The large refractor of the Radcliffe Observatory has an aperture of twenty-four inches, not eighteen inches. The University of South Africa does not possess an observatory, and there is no observatory in Cape Town other than the Royal Observatory. The twenty-four-inch photographic refractor of this observatory has an eighteen-inch visual refractor on the same mounting.

Neither the University of Michigan nor the Yale University has branches in the grounds of the Union

Observatory, Johannesburg. The University of Michigan southern station is on Naval Hill, Bloemfontein; the Yale University southern station is in the grounds of the University of the Witwatersrand, Johannesburg.

The southern branch of the Harvard Observatory is at Mazelspoort, about fourteen miles outside Bloemfontein.

H. SPENCER JONES
H. M. Astronomer

SCIENTIFIC APPARATUS AND LABORATORY METHODS

SHAKING MACHINE FOR ANALYTICAL WORK

THE world's industrial progress has been measured by a number of different inventions, especially applied mechanical devices. When time became very precious, as it is in our days, human ingenuity had to stretch its limits seeking some machinery to save labor and energy.

A chemist in his laboratory, for example, knows how to appreciate all his apparatus which make him able to carry on his research or routine work quite conveniently. The shaking process, which is sometimes an important factor in analyses, is a very tedious and time-consuming operation. Yet, using a properly constructed shaking machine, the process becomes easy and simple. The shaking machine will do good and rapid work in many instances.

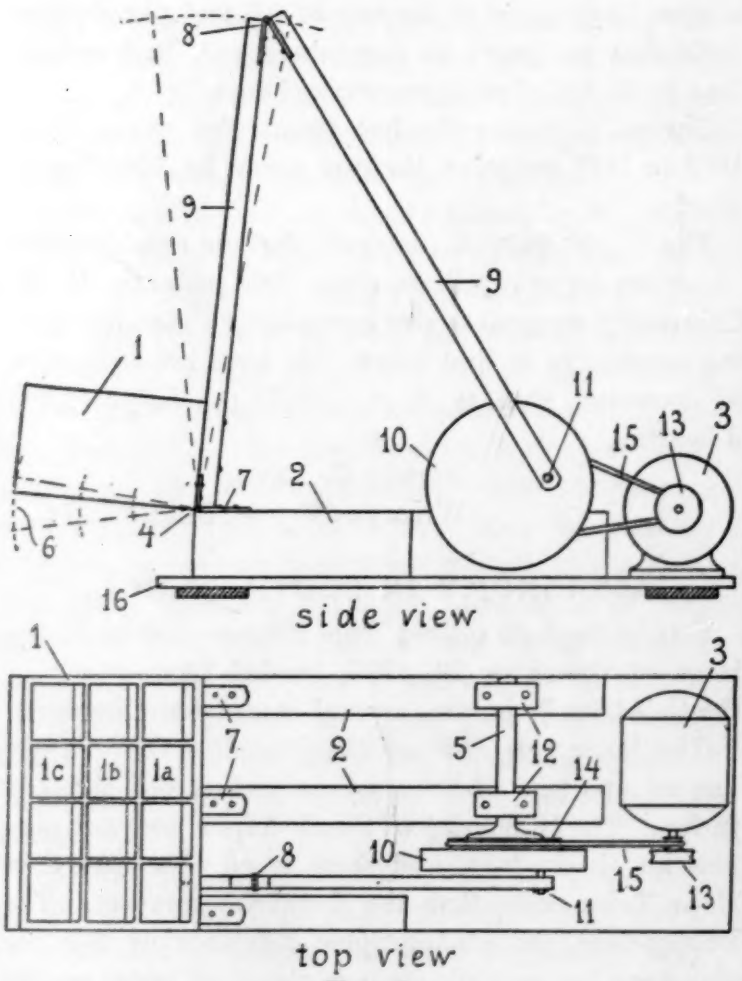

FIG. 1

The accompanying diagram represents one of the many useful shaking machines which have been designed and used in my laboratory of experimental embryology. This machine consists of a box with compartments (1) hinged to a base (2) and brought into a swinging motion by an electric motor (3) through a mechanical "system of four-links chain," where (4) and (5) are the axes of oscillation and rotation, and (6) is the maximum amplitude of swinging. Other parts of the machine are indicated: the hinges (7) and (8), shear legs (9), fly wheel (10) with crank and pin (11), bearings (12), motor pulley (13), driving pulley (14), belt (15) and base plate (16).

The shaking can be done in the box (1) with several samples under various amplitudes of swinging. The first row of compartments (1a) provides mild, the second (1b) moderate and the third (1c) intense shaking. If it is necessary, the amplitude of swinging in the machine can be easily adjusted by displacing the crank pin (11); and the speed of rotation can be regulated by the speed of the motor (3), using either a rheostat or several sizes of driven motor pulleys (13).

The machine described above, owing to its double action, or "swinging motion," might be particularly desirable in a thorough but gentle shaking or in any other special type of analytical work.

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AN APPLICATION OF STREAK AND HARDNESS USED IN CLAY GRADING

THE streak of a mineral is commonly used by mineralogists and geologists in its determination when the hardness of the mineral is less than that of the streak plate, but it is seldom that the mineral specimen itself serves as the streak plate for another substance and is thereby identified. This latter method, however, is used by workmen mining diaspore clay in Missouri to estimate the quality of the diaspore in the clay pits.

They have observed that when a lump of diaspore clay is broken by either a pick or "gad" (a steel wedge) a dark mark, the streak of the abraded steel tool, is left upon first-grade clay, whereas the inferior clay is only crushed or compressed where it is tooled. The workmen speak of the clay as "blacking" or "marking" well when the streak left upon the clay boulder by the iron tool is distinct.

Furthermore, it is known that the degree of "blacking," *i.e.*, the intensity and depth of color of the streak, varies directly with the alumina content of the clay. Since the value of high alumina clay is determined by its refractory quality and since this is dependent largely upon the alumina content, the depth of color of the dark streak is a valuable aid in estimating the quality of the clay. In practice a grader observes the "mark" made by the tool or his grading hammer, and quickly noting the fracture, texture and oolites present in the clay, sorts it into first grade, second grade or burley with a speed and accuracy that are surprising. An estimation of the alumina content to within 5 per cent. is regularly made, and under favorable conditions car-load quantities and even larger of one grade may be supplied with still less variation.

Although, upon a cursory examination, the test as outlined above might seem crude and undependable, its practicability is more evident after a review of some of the factors involved. Diaspore and burley clay are made up of the mineral diaspore, which contains 85 per cent. Al_2O_3, and a bonding matrix that is kaolin-like, its alumina content being in the neighborhood of 40 per cent. The hardness of diaspore is 6.5–7 (Dana), whereas that of kaolin is 2–2.5. It is evident that diaspore will scratch or cut steel, and that the kaolin-like matrix will not. Hence, the higher the diaspore content of the sample the more marked its abrasive action will be, the darker the streak of steel and the higher the alumina content. The clay functions both as a hardness element and as a streak plate. Quantitatively, clay containing 60 per cent. alumina and above will usually retain a mark, whereas that below 60 per cent. only packs or crushes to a slightly darker color and does not cut steel. An intense black to blue-black color is left upon clay running over 70 per cent. in alumina.

It might also be noted here that the clays higher in silica break with a more typical conchoidal fracture, those lower in silica with a more hackly break, similating, in this respect, the rhyolite-basalt fracture contrast. While the writer does not wish to indicate that the cause of the conchoidal fracture lies entirely in the high silica content he does wish to show that a relationship is suggested, and he points out the analogy to the igneous rocks because of its lithologic interest. A fuller discussion of the various means used in grading the high alumina clays of Missouri is published in *Brick and Clay Record,* October 8, 1929.

W. D. KELLER

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SPECIAL ARTICLES

AN AQUEOUS EXTRACT OF THE SUPRARENAL CORTEX WHICH MAINTAINS THE LIFE OF BILATERALLY ADRENALECTOMIZED CATS

IT is a well-established fact that in cats and dogs bilateral adrenal extirpation results in death within a short time, generally within eight to ten days. The animals exhibit a train of symptoms which has come to be recognized as typical of adrenal insufficiency. Experiments have shown, moreover, that death is due to removal of the cortical portion of the suprarenal complex and not to interference with, or loss of, the medulla or adrenalin-containing portion.

Administration of adrenalin to bilaterally operated animals has been repeatedly tried but without success, since animals so treated derive no benefit from the injections and die with characteristic symptoms of adrenal insufficiency. We have made numerous attempts to prolong the life-span of adrenalectomized cats by injections of adrenalin, employing various dosages, but in no case have we noted any improvement in the condition of the animals or been able to prevent the onset of symptoms.

During the past two years the writers have concentrated their efforts upon the cortex of the suprarenal and have prepared and tested a large number of different cortical extracts and have used over three hundred and fifty cats in the experiments. Varying degrees of activity were obtained from several of the preparations, and one lipid fraction revealed a considerable degree of potency. A brief report of the early work was published in the abstracts of the American Society of Zoologists.[1] Since the abstract was written, however, the lipid fraction was further fractionated and an aqueous preparation obtained which shows greater potency than the original lipid extract. This fraction is prepared in the following manner.

The suprarenal glands (beef) packed in ice are received from the slaughterhouse within twenty-four hours after the death of the animals, and the cortex dissected as free as possible from extraneous fat and medullary tissue. The ground cortical tissue is extracted at room temperature successively with 95 per cent. ethyl alcohol and 80 per cent. ethyl alcohol for varying periods of time, usually about three or four days, although we have obtained active extracts from material stored in 95 per cent. ethyl alcohol at room temperature for three weeks. The extractives are

[1] *Anatomical Record,* 44: 225, 1929.

concentrated separately in partial *vacuo* at a low temperature and the residues thoroughly extracted with benzene. The benzene is removed in partial *vacuo* at a low temperature and the lipoidal residue extracted with acetone. The acetone-soluble material, after removal of the acetone in a similar manner, is distributed between 70 per cent. ethyl alcohol and low-boiling petroleum ether. The alcoholic phase is washed repeatedly, the petroleum ether washings combined and the petroleum ether removed by distillation. The residue is again subjected to the distribution procedure. The petroleum ether washings are washed with 70 per cent. alcohol. The alcohol is removed as before, and to the aqueous concentrate sufficient distilled water is added to obtain the desired volume. In the present study we have arbitrarily made the dilution so that 1 cc represents 30 gm of freshly dissected cortical tissue. The extract is clarified and sterilized for subcutaneous injection by filtration through a Seitz filter. The solid content of this extract varies between five and ten milligrams per cubic centimeter. The extract is not entirely adrenalin free.

Present knowledge of the functional significance of the suprarenal cortex is so vague and uncertain that the only reliable criterion for testing the activity of a cortical extract is its effect upon the life-span of adrenalectomized animals. The criterion is severe and requires much time and effort, but at any rate it is an excellent index of potency.

All cats, both controls and extract-treated, were given uniform treatment. They were treated with a vermifuge upon first admittance to the laboratory—a necessary precaution since many animals in the vicinity of Princeton are heavily infested with hookworm—kept in thermostatically heat-regulated rooms, fed identical diets and operated by the same individual. At least seven days intervened between removal of right and left adrenals.

The average life-span of twenty-five bilaterally adrenalectomized control cats (untreated) was seven days; the maximum survival period of any animal in the control series was fourteen days; the minimum survival was four and one half days. Twenty-four hours following the second operation the first injection was given. Each animal received subcutaneously 0.5 cc to 1.0 cc of extract per kilogram of body weight daily.

Animals so treated remain in perfectly normal condition forty to fifty days, and can not be distinguished from normal unoperated cats. They eat, play, fight with one another and keep themselves sleek and clean. Most of our animals have shown weight increases during treatment. We have sacrificed several of our animals on the fiftieth day of survival in order to search for accessory adrenal tissue, but in no case have we found anything, despite the most exhaustive search.

In our earlier work with the lipid fraction which was injected in oil subcutaneously, considerable difficulty was experienced in keeping our animals much beyond forty or forty-five days, owing to accumulation of oil under the skin and abscess formation. The animals' coats became rough, the hair fell out in patches and their weight gradually declined. None of these difficulties have arisen in the experiments when the aqueous preparation of the original lipid fraction was employed.

The interesting point is that all healthy double operated animals survive and remain in normal condition when treated with the extract. Just how long cats so treated will survive we are unable to say at present, since our longest surviving animals are now in their eightieth day and in excellent condition. The experiments, however, justify the conclusion that we have successfully extracted from the suprarenal cortex of beef an active hormone which maintains the life of bilaterally adrenalectomized cats.

A detailed account of the various experiments will be published shortly.

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THE PRE-OLIGOCENE STRATIGRAPHY OF PORTO RICO

A RECENT geological survey of northeastern Porto Rico shows that the stratigraphic sequence suggested by Carlotta J. Maury in SCIENCE for December 20, 1929, needs emendation, at least so far as the formations which lie unconformably beneath the Oligocene-Miocene coastal plain are concerned.

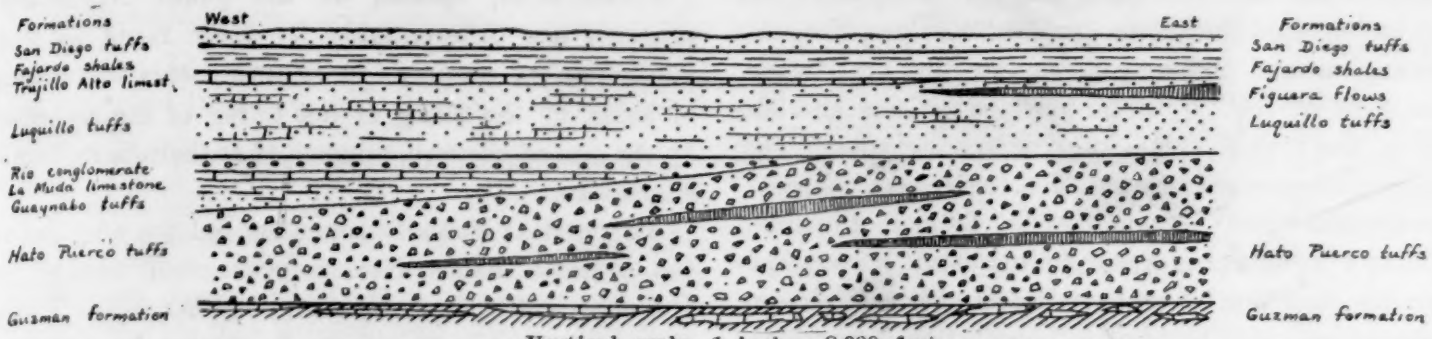
According to Dr. Maury, rocks of Eocene, Upper Cretaceous and possibly Lower Cretaceous ages are present. She regards the La Muda limestone as one of the youngest pre-Oligocene horizons, placing it in the Upper Eocene; and a similar, if less precise, position was given it by D. R. Semmes in 1919[1] on the basis of a small and inconclusive micro-fauna. The writer's structural studies have developed the fact that the bed lies stratigraphically beneath the Upper Cretaceous formations which Maury calls the Fajardo and Cape San Juan limestones.

Both Maury and E. T. Hodge[2] believe that the limestone south of Cidra lies at, or near, the base of the Porto Rican section, and they both place it tentatively in the Lower Cretaceous. This formation, however,

[1] D. R. Semmes, "The Geology of the San Juan District, Porto Rico," "Scientific Survey of Porto Rico and the Virgin Islands," Vol. I, pp. 74–75, 1919.

[2] E. T. Hodge, "The Geology of the Coamo-Guayama District, Porto Rico," *idem*, pp. 132, 192–193, 1920.

has been found to be the La Muda limestone, repeated on the flank of one of the several folds which rib the island. It lies neither at the base nor at the top, but slightly below the middle of the 10,000 to 11,000 feet of pre-Oligocene volcanics and sediments in this part of Porto Rico. The limited evidence available indicates that they are all Upper Cretaceous in age.



Vertical scale: 1 inch = 8,000 feet.

FIG. 1. Restored section of the Upper Cretaceous rocks between Bayamon and Fajardo, P. R.

It must be assumed that the designation of the rocks in the vicinity of Fajardo and Cape San Juan as limestones was an inadvertent slip, for the outcrops in this region consist of the Fajardo ashy shales and of a formation of stratified tuffs for which the name "San Diego" is herewith proposed. There are no limestones nearer than the valley of Rio Sabana, six miles to the west, where a thin wedge of the Trujillo Alto limestone is present.

The writer is not familiar with the structural and stratigraphic details in central and western Porto Rico, but the facts published suggest that some caution is necessary in formulating a geologic section which utilizes stratigraphic units from those portions of the island. Hodge placed the Rio Jueyes series in the Eocene on the strength of a single specimen identified as *Venericardia alticosta* Conrad,[3] and Maury apparently accepts his conclusion. Above the Rio Jueyes series lie the Coamo tuff-limestone and the Rio Descalabrado series, which both authors have necessarily referred to the Eocene. Yet, in the calcareous strata of this entire sedimentary series, which outcrops on the southern slopes of the Cordillera Central, G. J. Mitchell has reported abundant specimens belonging to the rudistid genus, *Radiolites*.[4] No better index fossil of the Cretaceous can be found, and in view of the presence of rudistids in the Rio Jueyes and Rio Descalabrado series, their assignment to the Eocene may well be questioned. The presence of Eocene rocks in any part of Porto Rico thus appears to be extremely dubious, although the possibility of their existence has not yet been completely eliminated.

At the present time the information in print is not adequate for the development of a stratigraphic succession that will apply to the pre-Oligocene rocks of the entire island. Determination of such a sequence is rendered triply difficult by the large intrusions which cut the section, by the complexity of the structure and by the baffling lateral gradation which some of the formations undergo. The accompanying restored section will illustrate the type of lateral change that occurs in northeastern Porto Rico within a distance of thirty miles. The geological age of the basal formation is not known; the others are Upper Cretaceous but do not include the highest members of the pre-Oligocene sequence.

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[3] *Idem*, pp. 193–194.

[4] G. J. Mitchell, "The Geology of the Ponce District, Porto Rico," *idem*, pp. 255–257, 1922.

THE PROPER TAXONOMIC CLASSIFICATION OF CERTAIN PYTHIACIOUS ORGANISMS

THE literature of mycology and plant pathology of the last few years contains many such phrases as "Pythium-like fungus causing this or that disease."[1] It is unfortunate to find in highly technical papers such expressions as the above, that tend to confuse rather than clarify the true taxonomic position of a group of organisms of great economic importance, since many of them are very aggressive plant pathogenes. This group of Pythium-like fungi is represented by such species as *P. gracile* Schenk,[2] *P. monospermum* Pringsh.,[2] *P. Butleri* Subram.,[3] *P. aphani-*

[1] B. B. Branstetter, "Corn Root Rot Studies," Res. Bulletin 113, Agr. Exp. Sta. Univ. of Missouri, 1927; C. W. Carpenter, "Pythium in Relation to Lahaina Disease and Pineapple Wilt," *Hawaiian Planters' Record*, 23: No. 3, pp. 142–174, 1920; C. W. Carpenter, "Morphological Studies of the Pythium-like Fungi Associated with Root Rot," Hawaiian Bulletin Exp. Sta. Hawaiian Sugar Planters' Association, Bot. Series, Vol. 3, Part I, pp. 59–65, 1921; C. W. Edgerton, E. C. Tims and P. J. Mills, "Relation of Species of Pythium to the Root Rot Disease of Sugar-cane," *Phytopath.*, 19: 549–564, 1929; Helen Johann, James R. Holbert and James G. Dickson, "A Pythium Seedling Blight and Root Rot of Dent Corn," *Jour. Agr. Res.*, 37: 443–464, 1928.

[2] Alfred Fischer, "Die Pilze: Phycomycetes," pp. 383–410 in Rabenhorst's "Kryptogamen Flora von Deutschland, Oesterreich and der Schweiz," IV. Abtheilung, Leipzig, 1892.

[3] L. S. Subramanian, "A Pythium Disease of Ginger, Tobacco and Papaya," Memoirs of the Dept. of Agriculture in India, Bot. Series, Vol. I0, No. 4, 1919.

dermatum (Edson) Fitzpatrick[4] and *P. arrhenomanes* Drechsler.[5]

This group of organisms has been called "Pythium-like" because it possesses, on the one hand, certain likeness to the true Pythium organisms, and on the other, certain dissimilarities. Fischer[2] recognized this fact and created the subgenera Aphragmium, Nematosporangium and Sphaerosporangium, the first two to take care of the "Pythium-like" organisms and the last of the true Pythium organisms. Schröter[6] later divided the genus Pythium into the genera Nematosporangium and Pythium, the former including Fischer's subgenera Aphragmium and Nematosporangium and the latter only the subgenus Sphaerosporangium. The basis of differentiation between the genera Nematosporangium, Pythium and Phytophthora lies in the morphological difference existing in their zoospore-producing organs.

There are three morphologically different organs, the prosporangium, emission collar and zoosporangium. The prosporangium serves as reservoir of the protoplasm intended for the development of zoospores, and the zoosporangium for the full development and discharge of zoospores. The emission collar separates the prosporangium from the zoosporangium and serves for the passage of the protoplasm from the former to the latter organ. The wall of the prosporangium is a continuation of and identical with the exterior wall of the hypha supporting this organ, whereas that of the zoosporangium is not, but constitutes a part of the so-called ectoplast or protoplasmic membrane of the prosporangium. The zoosporangium is of short duration; it emerges from the emission collar almost simultaneously with the flowing protoplasmic contents of the prosporangium and lasts until the zoospores are completely formed and have escaped into the surrounding medium.

The three genera may be differentiated on this basis, as follows:

Nematosporangium: Prosporangia not well defined, nematoid, alantoid or rarely subspherical, intra- and extra-marginal; emission collar very long; zoosporangia spherical, size variable; zoospores few to many.

Pythium: Prosporangia well defined, spherical to ovoid, mostly extra-marginal, rarely intra-marginal; emission collar short; zoosporangia spherical, size variable; zoospores few to many.

Phytophthora: Prosporangia well defined, lemon shaped to spherical, mostly extra-marginal of different sizes; zoosporangia developing within the walls of prosporangia; emission collar entirely lacking or rarely slightly developed; zoospores few to many.

Furthermore, species of the genus Nematosporangium are differentiated by their rapid or slow production of oospores in culture media and tissues of hosts, by the filling or not filling of the oogonium by the size of oospore, number of antheridia and other characters.

Fitzpatrick[4] strongly recommended the adoption of the generic name Nematosporangium instead of Pythium in *Rheosporangium aphanidermatum* Edson, but unfortunately this recommendation has not been adhered to by more recent workers.

The writer, on the basis of extensive studies on all the well-known "Pythium-like" organisms or Nematosporangium spp., and, in addition, on eight more new species obtained from diseased pineapple roots, is absolutely convinced that the morphologic differences existing between the Nematosporangium organisms, on the one hand, and the Pythium ones, on the other, are sufficiently significant to merit generic distinction.

It is proposed, therefore, that those organisms, known heretofore as *Pythium aphanidermatum* (Eds.) Fitz., *P. Butleri* Subramanian and *P. arrhenomanes* Drechsler be renamed as *Nematosporangium aphanidermatum* (Eds.) Fitz. comb. nov., *N. Butleri* (Subram.) comb. nov. and *N. arrhenomanes* (Drechsler) comb. nov.

Such organisms as *Pythium gracile* de Bary, *P. dictyospermum* Rorciborski and *P. monospermum* Pringsheim have been placed already by Schröter in the genus Nematosporangium.

The writer is preparing a paper which will soon be published wherein eight new species of this genus are described and the taxonomic features of this genus are thoroughly discussed.

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[4] H. M. Fitzpatrick, "Generic Concepts in the Pythiaceae and Blastocladiaceae," *Mycologia*, 15: 166–173, 1923.

[5] Charles Drechsler, "*Pythium arrhenomanes* n. sp., A Parasite Causing Maize Root Rot," *Phytopath.*, 18: 873–875, 1928.

[6] J. Schröter, "Pythiaceae," in Engler-Prantl, "Natürliche Pflanzenfamilien," pp. 104–105, 1897.

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